

SCIENCE.

FRIDAY, MARCH 11, 1887.

COMMENT AND CRITICISM.

ALTHOUGH CONGRESS HAS NOT ORDERED that the weather-bureau shall be transferred from the signal corps of the army to some civil department, the steps that were taken towards the transfer give strong assurance that it will be made next year, when it can be undertaken more deliberately. The action was briefly as follows: the house bill No. 5190, to create a department of agriculture and labor, received several amendments in the senate, among which the sixth had for its object the transfer of the weather-bureau from the signal office of the army to the new department on the 1st of next July. Although several senators voted on Feb. 23 against this amendment, because they thought the action was too precipitate, it had a majority of thirty-seven to fifteen, with twenty-four absent. It provided that the second lieutenants and the subordinate members of the corps should be transferred to the new department, without changing their work or their pay; that the rank of commissioned officers of the signal corps should not be affected by the transfer; and that the chief signal officer should remain in charge of the bureau after the transfer until a director should be appointed for it. The bill then returned to the house, where, according to the reports we have received, it would have certainly been passed as amended, had not an unforeseen obstacle arisen. The President, it seems, does not desire an additional member in his cabinet: the bill was therefore referred back to the committee on agriculture by his friends in the house, and at this late date in the crowded session it could not again be reached, not being 'privileged business.' So the matter is dropped for the present.

This postponement is, on the whole, not to be regretted. It is quite clear that the failure to make the change was not due at all to a belief that it ought not to be made. Senator Edmunds offered the only considerable objection to the transfer during the debate on the amendment. It was clear to him, "that the only way to have

an effective organization is to have it under military control, so that a man cannot resign because he gets miffed about something, but he must do his duty." This mistaken impression found few if any supporters. It seemed to be generally understood that the loss of individuality and complete submission to authority, which constitute the essence of the military spirit, are out of place in a service that wisely makes open declaration of its need of intelligent personal action by calling on college graduates to enlist in it. Senator Dawes thought every one agreed that the service "ought to be transferred to the civil department of the government," but believed that the transfer ought to be made more deliberately than was contemplated in the amendment. Senator Hale expressed the same views, and these two joined Edmunds and others in voting against the bill. But their favorable votes may be expected next winter, when perhaps less political and more appropriate surroundings may be chosen for the weather-bureau than it would have found in the proposed new department.

In the mean time the position of chief signal officer is given to Captain Greely, who is thereby promoted to be a brigadier-general, the senate having confirmed the President's nomination at the last moment. So great an advance in rank is unusual, and may be attributed in part to recognition of arctic heroism, — for surely the preservation of a complete series of records under the most difficult and tragic circumstances was a splendid achievement, — and possibly in part to the feeling that the office should be given to some one already in the service, rather than to some colonel who stood, indeed, nearer in the line of promotion, but who had had no experience in the weather-bureau. But the failure of the deficiency bill makes the position of chief signal officer an arduous one for the next year, for it is a thankless duty that involves reduction in some of the essentials of the service. It is to be regretted that the new chief was not given at least the best opportunity of showing his powers. The remedy for unsatisfactory weather-predictions is not likely to be found while the service is thus embarrassed.

SMALL-POX IS SAID to have appeared recently at Holyoke, Mass., among the rag-sorters of the paper-mills, presumably contracted from handling infected rags. There are two points of interest in connection with these cases, on which we should like information : first, were the suspected rags domestic, or foreign ? and, second, were the rag-sorters vaccinated, and, if so, when was the operation last performed ? The necessity for disinfecting foreign rags has been so much discussed of late years, that every instance of this kind should be thoroughly investigated, and the results reported in detail.

THE NEWEST MONOGRAPH of the American economic association is, like its predecessor, a study of co-operation. But the field of observation is shifted from Minnesota to New England. The author, Mr. Edward W. Bemis, keeps himself in the background throughout, only occasionally in the tone of his treatment giving indications that he is a believer in co-operation as a remedy for many of the existing and much-commented-on labor-troubles. The monograph is contained in one hundred and thirty-six pages, and gives a succinct account of the various co-operative and profit-sharing enterprises undertaken in New England, from the time of the hapless Brook Farm (1842-47) to the introduction of profit-sharing into a Boston newspaper establishment at the beginning of the present year. Distributive and productive co-operation are treated separately ; for they are very different things, the former being the simpler, more easily managed, and requiring a far smaller amount of capital than the other. The conditions of productive co-operation are more complicated and involved than those of co-operative distribution, and therefore the latter comes first in the order of time.

In New England the development of the co-operative movement seems to have been continuous, for members of the Brook Farm community were prominent in the co-operative enterprises of the Sovereigns of industry and the Knights of labor ; and the various protective unions, and so forth, seem to have grown one out of the other. The Sovereigns of industry, organized in 1874, assert that they were the first to introduce the Rochdale plan into this country, but members of the New England protective union claim to have established co-operative stores on the Rochdale plan in Boston as early as 1864. The peculiarity of the Rochdale

plan is, as is well understood, that goods shall be sold at the retail market-price, and any profits that remain, after an allowance has been made for a reserve fund and interest on capital, are apportioned to the customers on the basis of their trade for the period since the preceding distribution ; it is permitted to stockholders, however, to receive a larger dividend than is paid to outsiders.

As Mr. Bemis himself says, the record of the early years of the co-operative movement contains more failures than permanent successes. A comparison of the causes of failure, as adduced by the author, shows a curious agreement, even in the case of enterprises undertaken under conditions quite diverse. The New England protective union, for example, went along from 1847 until 1852, when it had as many as four hundred and three subdivisions, of which one hundred and sixty-five reported total sales the previous year of \$1,696,825.46. No attempt had been made to secure large profits ; goods were sold at as near the cost-price as was deemed consistent with safety ; and the members were satisfied with six-per-cent dividends on the stock. But there was frequently a simultaneous increase, both in the price of goods and in the amount of dividends to the comparatively small number of stockholders. "Many stores thus ceased to be co-operative, and the stock passed into the hands of a few of the more enterprising or well-to-do."

It is the same story all the way through. "The underlying causes of all co-operative failures are lack of intelligence and of the spirit of co-operation." After a time there is a disagreement ; the management is declared to be arbitrary ; the store-keeper is paid too much ; it is asserted that better bargains can be made outside. This creates lack of confidence, and to restore it there is a departure from the cash principle, or an increased dividend is declared. The result is disastrous. Most of the above sentences are culled from Mr. Bemis's history of the various concerns, and not a few of them are in substance the words of such believers in co-operation as Holyoake, George E. McNeill, and others. They involve the admission of all that the friendly critics of co-operation claim ; that is, that it is an ideal scheme, suited to a perfectly homogeneous community, the members of which are willing to make extensive temporary sacrifices in order to its ultimate success. For this

reason it cannot become a universal economic system. The same human nature that interferes with so many other beneficent schemes, interferes with this. "Co-operative concerns fail because of a failure to co-operate," is the universal verdict.

It is but fair to point out that the data gathered from the latter part of the period of which Mr. Bemis writes, are more favorable to co-operation. Increased experience may have something to do with this. From the tables compiled by the author, it appears that productive co-operation in twenty companies in New England shows a business of \$1,000,000 a year; co-operative stores have a trade of over \$1,750,000; co-operative creameries do a business probably of \$1,000,000; and about \$3,250,000 are invested in co-operative banks. So that, apart from co-operative insurance companies, the annual business of the co-operative companies of New England amounts to about seven millions of dollars. In Massachusetts the conditions seem to be specially favorable to co-operative companies, as the state has a general law for their incorporation. The capital stock of such a company is limited to \$100,000, and must be more than \$1,000. No one person can hold more than \$1,000 worth of stock, or have more than one vote. It is further provided that there shall be an annual distribution of profits among the workmen, purchasers, and stockholders; but ten per cent of the net profits must first be set aside for a contingent or sinking fund, until a sum equal to thirty per cent of the capital stock shall have been accumulated. The word 'co-operative' must form part of the corporate name, and shares to an amount not exceeding twenty dollars are exempt from attachment and execution. The credit of the company and security of the stockholders are further increased by a full report made annually to the secretary of state. The last section of the monograph is devoted to profit-sharing, and brings forward some interesting instances in which it has been put in operation. The best known, perhaps, is that of the Peace Dale manufacturing company, where profit-sharing was begun eight years ago. An average dividend of four per cent on the wages was paid to the workmen for four years, but since 1883 no dividend has been declared. From none of the cases of profit-sharing adduced by Mr. Bemis can we deduct any arguments which meet the objections of Mr. Aldrich, on which we commented last week.

THE EXPLORATION OF THE WELLE.

SCHWEINFURTH has recently sent a letter to the editor of *Le mouvement géographique*, from which we take the following abstract: The Welle-Makua has been crossed by Junker at six different points. At Ali Kobo, in the country of the Basange, his farthest point west, the river attains such dimensions that he could not estimate its size, particularly as it is blocked up by islands, which are not only densely populated and highly cultivated, but afford ample room for herds of elephants which abound there. Junker could not stay here longer than four days. Only a comparatively short distance from the Kongo, he was compelled to return, as Lupton Bey, the governor of the Egyptian province Bahr-el-Gazal, sent him word of the rapid spreading of the mahdi's power. Eight days' journey beyond the extreme point reached by Junker, the Mbomo empties itself into the Welle. The Mbomo runs east and west, and has many tributaries, which come from the watershed between the Kongo, the Shari, and the Nile. In February, 1883, Junker reached Abi Kobo. Junker's 'Nepoko' is probably the upper course of the Biverre. He heard another river mentioned, the Nava, which, however, he did not see. Schweinfurth is of the opinion that it may be the upper course of the Biverre, while the Nepoko may be that of the Mburu. The quantity of water in the latter is, however, so small that its source must be looked for farther west.

Wauters's hypothesis of the identity of the Welle and Obangi becomes very probable by Junker's new discoveries, as will be seen by the accompanying sketch-map. Wauters supposes that Grenfell, who explored the latter river, passed by the mouth of the Welle without seeing it. The remarkable form of the right bank of the Obangi, the appearance of the first hills at the place of the supposed confluence, the dotted lines by which Grenfell indicates the left bank at this point, and the suddenly increasing shallowness of the river, all support Wauters's hypothesis. This new information is of great importance for the progress of Stanley's expedition for the relief of Emin Pasha. He may either ascend the Obangi and Welle, the Biverre-Nepoko, or start from Stanley Falls. It is doubtful whether there are any rapids in the Welle that might obstruct his passage. As Grenfell passed the rapids of the Obangi in latitude $4^{\circ} 30'$ north without any difficulty, and those of the Kongo at Rubungu do not prevent the passage of steamers, it is possible that no serious difficulties of navigation exist.

We may be allowed to call to mind at this place the sources of our former knowledge of this district. After Schweinfurth's discovery of the

Welle, Nachtigal was the first to give some new information. In 1875 he published a map from his surveys and from information obtained in Dar For and Wadai. Junker explored, in 1876 and 1877, the western tributaries of the Bahr-el-Abiad. In the same years a Greek physician, Panagiotis Potagos, travelled over a great part of the district. As, however, he made no astronomical observations, and his itinerary is very primitive, the results of his journey are not reliable. This is still more the case with Bohndorff's journeys. This man, a goldsmith, who had been in the service of General Gordon, travelled in the region of the head waters of the Welle. Later on, when Junker started on his second journey, he took Bohndorff for his servant, and in January, 1880, they left Khartum. The first summer was spent in the Niam-Niam country, and since that time Junker has travelled in Mombuttu and in the district of the Welle and the other rivers running west. Lupton Bey and his agents made many important journeys, the expedition of Rafai Aga being of particular interest. He is said to have reached the lake on the Lokoi. The north-western tributaries of the Bungu, as shown on the sketch-map, are from Flegel's reports, who learned about them on his journey in Adamaua. The central part between the regions traversed by Flegel, Nachtigal, and Junker, is still totally unknown.

THE HEALTH OF NEW YORK CITY DURING JANUARY.

THE population of New York City at the beginning of 1887 may be approximately stated to have been 1,461,466. The deaths during the month of January from all causes were 3,507, which is but 5 more than during the preceding month, although the population was greater by more than 3,000. Of this number, 140 died on the 5th, the greatest mortality of the month (see page 228). Diarrhoeal diseases caused 48 deaths, a reduction of 17 as compared with December, and the lowest mortality from this cause since March, 1886. The deaths of children under five years of age amounted to 1,523, differing but little from the preceding month. Consumption caused 524, diphtheria 204, and scarlet-fever but 46 deaths. The mortality from the last-named disease was double that of December. In November there were recorded 166 deaths as due to measles. In December this increased to 271, and in January the mortality rose to 294, exceeding by no inconsiderable figure the combined deaths from diphtheria and scarlet-fever, emphasizing, what we have already directed attention to, that measles is not a trivial disease, but one in regard to which all precautions

relating to isolation and disinfection should be promptly and thoroughly taken and maintained.

The maximum temperature of the month, 62° F., was reached at 4 P.M. on the 23d. This was nearly ten degrees above the average for the past ten years. The lowest point reached by the mercury was 4° F., at 12 P.M. on the 18th, and again on the 19th at 2 A.M. The average for the decade is 3.1° F., although during the same month of 1879 it fell to -4° F., and in 1882 to -6° F. The rainfall for January was 4.42 inches, included in which are 6.625 inches of snow. The average rainfall for this month for the ten years commencing 1878 is 3.82 inches, so that more than the average fell during January. The largest amount of snow which fell during this period in the same month was 17.5 inches, in the year 1882. Since then, in but one year, 1885, has less snow fallen than during January of 1887: the average has been nearly 10 inches. There were four snow-storms during the month. In that which occurred on the 5th and 6th, 2 inches fell; that of the 9th and 10th resulted in a fall of 4 inches; while the others were insignificant.

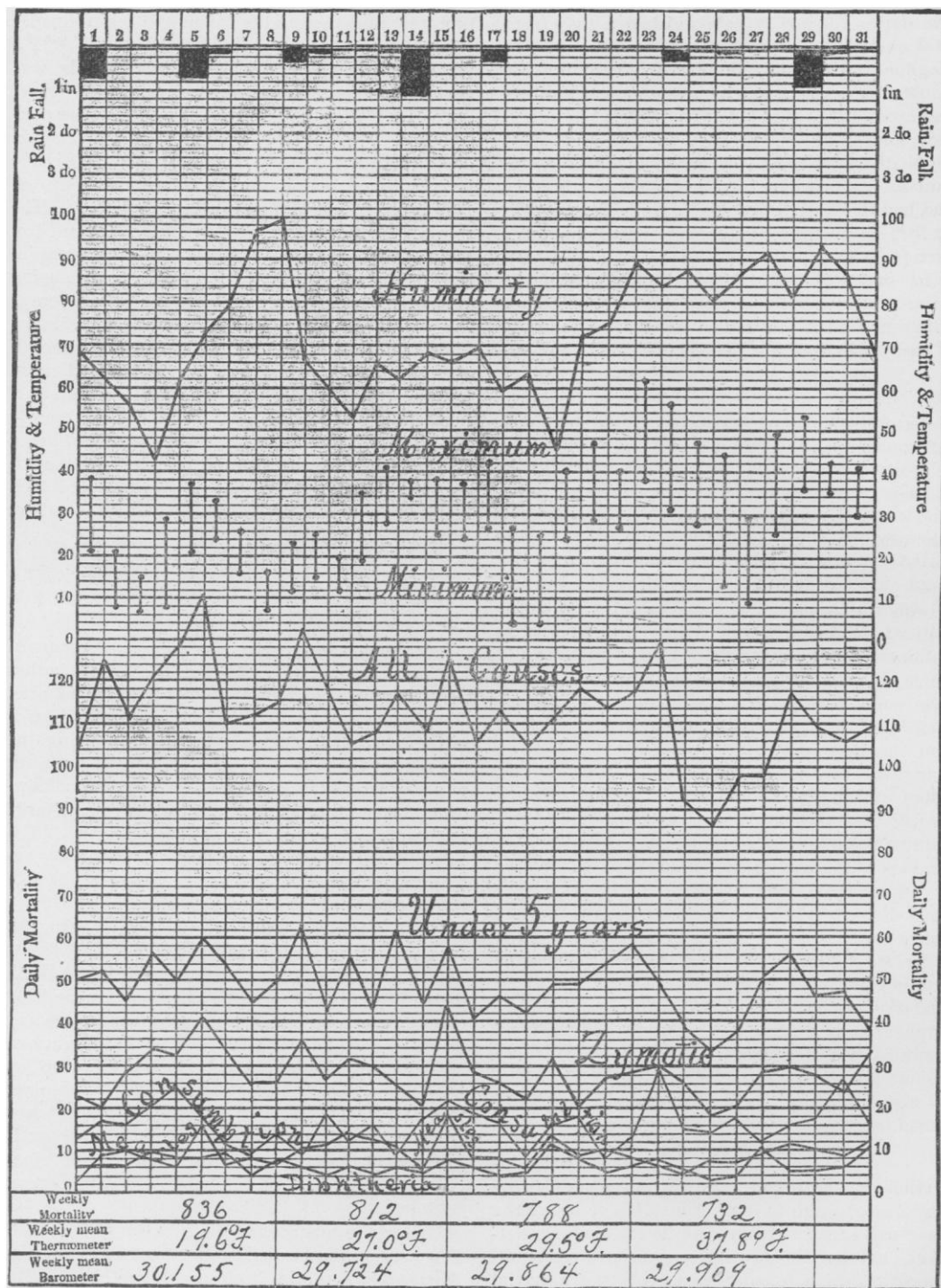
GEOGRAPHICAL NOTES.

Africa.

Dr. Hans Schinz gives the following report of the present state of Lake Ngami. The lake has not dried up, but is gradually decreasing in size. The Okavango, or Ombuenge, forms, north-west of the lake, an extensive swamp, and during the dry season the lake receives only a small quantity of water from it. During the rainy season, however, the small brooks swell up and form a large stream, which empties itself into the lake. The Tamulakan, which branches off from the Ombuenge in latitude 18° 40' S., empties itself into the Botelet, not into the Zambezi.

Gottl. Ad. Krause has succeeded in entering the territory south of Timbuktu. Since Barth's expedition in 1853, no white man has visited this district. On the 7th of July, Krause left the large city of Salaga on the Volta, and later on reached Mosi, whence he started on Oct. 26 for Timbuktu. The district through which he has travelled has been known only through information obtained by Barth. Our knowledge of the district between the fabulous Kong Mountains and the most northern part of the Niger is still extremely imperfect, being founded only on information obtained from natives.

Under the auspices of the secretary of state of France, Camille Douls is going to explore the Wad Draa, which empties itself near Cape Noon. This periodical river runs at some distance along the



south side of the Anti-Atlas, and drains its southern slope. It was crossed by Leopold Panet in 1850, about fifty miles above its mouth; by Si Bu-Moghdad in 1861, about twenty-five miles lower. Rabbi Mordochai followed one of its tributaries, and crossed it at the same place where Lenz did in 1880, about 120 miles above its mouth. Panet and Bu-Moghdad travelled very hurriedly, with a large caravan coming from St. Louis, on the Senegal, and had no chance for making many observations. In 1828 Caillié ascended its upper part on his return from Timbuktu. Douls intends first to visit Wad Sus, which is situated between the Anti-Atlas and the High Atlas. Rohlfs ascended the valley in 1862, when he explored the upper part of the Wad Draa and the Oasis Tafilet.

America.

Letters from Europe give some more particular information of the object of Dr. K. von den Steinen's expedition to Brazil. Three years ago he and Dr. O. Clauss surveyed the whole length of the Xingu. Von den Steinen intends to complete this work by exploring its sources. He will start again from Cuyaba. As on his former expedition geographical researches formed the main object of the journey, he could not make a long stay among the interesting tribes of the upper Xingu. Nevertheless he obtained ethnological information of great importance. On the present expedition he proposes to live some time with the Indians of that district, who have never been in contact with whites, and therefore are of particular interest for ethnologists. Dr. P. Ehrenreich, who has studied the tribes of Rio Doce, and made valuable anthropological observations during his journey, and the painter Wilhelm von den Steinen, will be his companions. This expedition, which consists exclusively of scientists who are thoroughly acquainted with the field of their researches, will yield valuable results.

Mr. H. N. Ridley, assistant to the British museum, is going to visit Fernando Noronha, the lonely island off the Brazilian coast. The Brazilian government has granted him permission to make botanical and zoological collections on the island, though generally visits of strangers are prohibited on account of a colony of convicts being established there.

Polar regions.

Gilder has returned from his journey to Hudson Bay, and given up for the present his plan to reach the north pole by this route. We pointed out last week that the difficulties he would encounter were almost insuperable, and are glad to learn that he reached the same conclusion. Gilder arrived at

Selkirk, near Winnipeg, March 2. According to his own account, after leaving Winnipeg last fall, he had a very unpleasant voyage to York Factory, occupying two months. He was unable to get a boat all the way, and had to proceed in a canoe, getting Indians to bring his supplies along. He reached Fort Churchill too late to catch a Hudson Bay boat for Nottingham Island, and, as he would have had to stay several months about Fort Churchill without occupation, he decided to return to New York to transact some business, after which he will leave in time to catch the next Hudson Bay boat, several months hence, or else take a whaling-vessel bound for the northern seas next summer. He left his companion, Griffith, at Fort Churchill, with instructions to take the stores and proceed to Nottingham Island by the first Hudson Bay boat. Gilder promised to join him there. It is to be hoped that he will give up the Hudson Bay route for good, and take a Scotch whaler going to Smith Sound instead. The route from Fort Churchill to Lancaster Sound by boat and sledge is impracticable, and ought not to be attempted by an explorer who wishes to visit the extreme north.

General.

Charles A. Schott has continued his study of the observations on terrestrial magnetism in America. In his former papers, which were published in the annual reports of the coast and geodetic survey for the years 1880-82, he treated the declination. The present paper — Appendix 6 to the report for 1885 — contains a large collection of observations on the magnetic dip and intensity. The collection of data is very complete and clearly arranged, so that it is easy to find the elements of any desired place. It will be of permanent value to the student of terrestrial physics. Schott discusses this large collection of data in order to ascertain the secular change of the magnetic dip and intensity, and uses the results of his researches, with due reserve, for the construction of charts of the United States showing the lines of equal magnetic dip and intensity. His scrutiny of the observations leads him to the conclusion that it is impossible at the present time to give a detailed map of this kind. The observations of most places are made at too long and irregular intervals, and are not sufficiently reliable. Therefore he gives only a general map of the course of these lines. The belts of stationary dip and intensity, which are indicated in the maps, showing the boundary between increasing and decreasing dip and intensity, are of special interest. The belt of stationary dip runs through the Strait of Florida, crosses the Mississippi just above its delta, and then turns again south, passes through central Texas, through

northern Mexico, crosses the Gulf of California, follows the coast of southern California, and passes out to sea off San Francisco. South of this belt the dip is increasing; north of it, it is decreasing. The curve of the secular change of the magnetic dip, though generally decreasing, had a secondary maximum about 1860. This subordinate extreme has been passed north of the belt, but has not yet been reached south of it. The magnetic intensity is also decreasing, and reached a subordinate maximum in 1870. Since then it is again decreasing. On the map showing the lines of equal horizontal force, Schott has marked the approximate situation of the region of stationary horizontal intensity. It runs from north-west Florida through Georgia, Tennessee, Missouri, Nebraska, Wyoming, and western Montana. South of this belt the horizontal force is decreasing; north it is increasing.

G. Hellmann has discussed the statistical data on damage done by lightning in Sleswick-Holstein, Baden, and Hesse, which are contained in the reports of the insurance companies. He finds the danger from lightning, though generally increasing, to be decreasing in certain districts. The danger becomes less the more closely the houses are clustered. The petrographical character of the ground is of great influence. If the danger from lightning upon calcareous soil be represented by 1, 2 will represent the danger upon marly, 9 upon sandy, and 22 upon clayey soil. No explanation can be offered for the fact that, among trees, oaks are struck most frequently. If the danger for beeches be 1, that for pine is 15, for oaks 54.

NOTES AND NEWS.

In a report by Passed-Assistant Surgeon T. H. Streets, U.S.N., of the U. S. coast survey steamer C. P. Patterson, surveying in the waters of Alaska, after referring to the vast forests of spruce, cedar, and hemlock which clothe the shores and mountains and islands of south-eastern Alaska with everlasting verdure, and alluding to the herring, cod, and halibut which inhabit the deep waters, the immensity of the schools of salmon is illustrated by the following account of what he saw at Naha: "To illustrate how immense are the schools of salmon, I will relate what I saw at Naha, where they crowded into a stream of fresh water in such numbers as to materially impede the progress of our canoe. Bruised, lacerated, and killed in attempting to surmount the falls that obstructed their course, suffocated in the jam below, where the water was awork with them, with backs and dorsal fins pro-

truding, their dead bodies lay two and three deep along the shores of the stream, and for fifteen to twenty yards from the water's edge, where they had been left by the receding water. The mouth of the stream was obstructed by a wire trap held to the banks by a wire fence. The trap, at the time of our visit, was raised to allow the fish to enter the stream. The wire fence was broken down by the weight of the mass of dead fish drifting against it, and many must have been carried to sea by the tides and currents. The air was offensive with the odor of the decaying carcasses. Flocks of ravens and gulls fed upon the dead, and the bears fattened upon the living; yet sufficient numbers overcome the high falls yearly to provide for the annual return of the swarms. A large fishery is located there, which also does its part to reduce their numbers. It is a blind instinct which leads migratory fishes to return to the streams where they were hatched; and Nature is prodigal with her forces in carrying out her plans."

— The signal service will be seriously crippled by the failure of the deficiency appropriation bill. The chief signal officer says, "It is now impossible to remove a man, even to discharge or recruit him, or to replace those who are dead or dangerously ill." The term of service of a number of men has expired, but they must remain in the corps from lack of money to send them to their homes. The telegraphic reports of cold waves, storms, warnings, etc., must be discontinued at a number of important points, as the funds on hand for that purpose are nearly exhausted.

— The new German *Centralblatt*, devoted to bacteriology and parasitology, continues to furnish its readers weekly with records of recent researches on these subjects. We understand that Dr. G. Sternberg will confine himself to reporting American original work on micro-organisms, and that Prof. R. Ramsay Wright, Toronto, has undertaken to furnish a similar account of papers published in America on animal parasites and on epidemics occasioned by them. Professor Wright will be obliged to authors for extras of such papers, which will be promptly noticed in the *Centralblatt*.

— The annual consumption of cocoa is 80,000,000 pounds, produced principally in the West Indies and South America. France consumes 26,000,000 pounds; Spain, 16,000,000; England, 14,000,000; and the United States, 8,500,000. Since 1860 the consumption of cocoa in the United States has increased sixfold; during the same period, that of coffee and tea has not quite doubled.

LETTERS TO THE EDITOR.

*.*Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

The failure of foreign trees on American soil.

ALLOW me to enter a respectful protest against the sweeping judgment of Professor Sargent in condemnation of foreign trees, which you publish approvingly in your issue of March 4. Though there is, no doubt, a great difference between the climate of this continent and that of Europe, and though unquestionably tree-growth is most dependent upon climatic conditions, yet it would be unwarrantable, from its failure in one place or even several places in this country in ornamental plantations, to generalize upon the adaptability of an exotic species for forestry use. It seems to be generally overlooked, if not unknown, in this country, that forestry and arboriculture, or tree-planting as practised by the horticulturist or landscape-gardener, are not the same thing, but in their objects, and consequently in their methods and results, are entirely different. While in ornamental planting the individual tree is the object, and its form in its unity and the development of its beauty is the aim of the planter, forestry has to do with an aggregate of trees, which, properly placed and grouped together, grow and develop very differently from the single tree, or even group of trees, on the lawn. The European larch, even in its native country, does not make a desirable lawn-tree in every locality, and, coming originally from the highest mountain elevations, even as a forest-tree, it requires, when grown upon the plain, particular conditions and special management to secure a thrifty growth, and the quality and quantity of timber for which the tree is noted. I have often pitied those in this country who have expected these results without paying attention to the requirements of the tree. As to the Norway spruce, of which Professor Sargent speaks so disparagingly. I have not seen a finer ornamental conifer of its kind on this side of the Atlantic; and though, as is the case with all the conifers, a time arrives when it loses its peculiar beauty, I doubt whether it does so sooner than any others, while, as a forest-tree, it needs only proper conditions and management, I venture to say, in order to attain the size and quality which it shows in its native country. Plant the Norway spruce in dense groves, on a northern or north-western exposure, with the European larch sparingly interspersed, and no planter will live long enough to see these two, thus united, fail in their onward development.

The Scotch-pine, on poor but deep sands on the western prairies, I am sure will make useful timber sooner than the white-pine. The white-pine was introduced into Germany on large areas about ninety years ago. Growing with great rapidity, and yielding astonishing quantities of wood per acre, the quality of the wood was found to be very inferior until recent years. Experiments have lately shown that the white-pine requires ninety years to make wood of as good quality as the Scotch-pine will produce in seventy years under similar conditions, just as different grains will require different lengths of season in which to mature. These experiments and the many similar ones which could be cited should teach us to be chary of generalizations upon our scanty experiences in forestry in this country.

Of the European willows, so far as osier-growing is concerned, only one, *Salix purpurea*, seems to

have been found adapted to our climate, while several native ones promise success if properly treated.

While I am a most earnest advocate of seeking for the best in that which we have ourselves, and while I advise the planting first of our native trees, with a special study of their requirements, I must deprecate any know-nothing movement against the good things which we may import. Especially let us remember that New England constitutes, territorially and climatically, but a very small part of our country, and that conclusions drawn from experiments there may not be applicable to other portions of it.

B. E. FERNOW.

Washington, March 7.

Inertia-force.

I had thought that my pamphlet, 'Elementary ideas,' etc., might awaken discussion, and possibly bring about a better understanding among teachers of physics as to the interpretation of certain familiar terms. The discussion has evidently begun. Let us not despair of the better understanding.

Having made, however, one direct attempt to explain to Professor MacGregor my use of the term 'inertia-force,' with the sorry result of disgusting him by the use of "language which is not the current language of dynamics," I shall for the moment adopt a different course, and find a little fault with his way of stating things.

Professor MacGregor accepts fully the doctrine stated by Maxwell in a passage quoted in my first letter, that "all force is of the nature of stress, that stress exists only between two portions of matter," and that "the stress is measured numerically by the force exerted on either of the two portions of matter." I will undertake to show wherein his reasoning seems to me to be inconsistent with this doctrine. He takes my illustration of a railway-train which is being set in motion by a locomotive, and says, "If F is the pull of the locomotive, R the frictional resistance, M the mass of the train, and a its acceleration, we have undoubtedly, by Newton's second law of motion,

$$a = (F - R) \div M."$$

To this every one will agree. Now, with Professor MacGregor's permission, I will put this equation in the form

$$F = R + aM.$$

F is, by his own statement, a force, — the force exerted by the locomotive on the train. By the doctrine stated by Maxwell, which Professor MacGregor accepts, the force exerted by the train on the locomotive is also equal to F . It is therefore equal to, and may be expressed by, the terms $R + aM$. Now, one part of this force, the part R , is accounted for by the resistance of friction transmitted through the train to the coupling of the locomotive. How shall we account for the other part of the whole force exerted by the train on the locomotive, the part aM ? I call it the *inertia-force*, — the force, or resistance, which the train, by virtue of its inertia, exerts on the locomotive which is setting it in motion. I think I can be persuaded to drop the term 'inertia-force,' if a more accurately descriptive one can be adopted; but Professor MacGregor, if I understand him, does not object to the term merely. He denies that the train offers any resistance by virtue of its inertia. But in

denying this he seems to me to reduce the force exerted by the train on the locomotive to the quantity R alone; and since R is less than F , the pull exerted by the locomotive on the train, he thus abandons the doctrine that "all force is of the nature of stress," and that "the stress is measured numerically by the force exerted on either of the two portions of matter."

The quotation which Professor MacGregor makes from Poisson I shall not attempt to discuss at length; for I am not familiar with his writings, and do not know exactly what meaning he attached to the word *résistance*. If he used this word as I understand Professor MacGregor to use it, to indicate an *opposing force*, and if he was at the same time committed, as I understand Professor MacGregor to be, to the view that one force always implies an equal and opposite force, then I can only say that I think Poisson was wrong in one part or the other of his doctrine.

E. H. HALL.

Cambridge, March 5.

Comparative taxation.

While I cordially accept all Mr. Henry B. Gardner's statements in regard to the insufficiency of my study of the comparative taxation in Europe and America, I cannot accept his conclusions. He says, in fact, "The inadequate scope of the work has in large measure destroyed the value of the study." To this I cannot agree; and my witness is Mr. Gardner himself. My work has brought out his intelligent criticism, and has turned the attention of himself and of very many other persons to the importance of developing the science of comparative statistics, which is what I have aimed at.

It is very true that I have not attempted to compare the relative taxation of cities, towns, and other subdivisions of states in Europe with those of America; it is very true that some of the cities of this country are excessively taxed as compared to those of Europe: all the more reason for a complete study of the subject. Where are the materials for such an investigation? I have given, to the best of my ability, the relative burden of *national* taxation. I stated that this part of the taxation of countries should be considered separately from that of the towns and cities, for the reason that in Europe a very large part of the national taxation is expended for *destructive* purposes or for the support of privileged classes; while, with the exception of a few cities in this country, the revenues derived from local taxation are paid out for *constructive* purposes both there and here; and on the whole, in spite of the cumbersome nature of the collective work of cities, counties, and towns, the people of this country get about seventy-five cents' worth on a dollar for what they pay in municipal taxes.

Moreover, although Mr. Gardner may not be able to find exact returns of taxation in European countries corresponding to the *per capita* figures which I have submitted, yet I claim to have proved them after as complete examination as is open to a private and unofficial person who does not read German. I hold that the revenue of state forests, mines, and other instrumentalities of subsistence which are often controlled in Europe by governments, constitute as true a tax upon the people as if they had been assessed directly upon their property; and I am of opinion that I have understated the burden of national taxation in

Europe rather than overstated it. Suffice it that the figures have attracted attention; and it may be that within one, two, or three years a complete comparison of national as well as state, county, and town taxation may become possible. I should be glad to see Mr. Gardner try his hand, not so much in criticising my work, as in preparing more accurate and more complete tables.

EDWARD ATKINSON.

Boston, March 5.

On the flight of birds.

The wing is extended upward from the horizontal position by the deltoid and the latissimus dorsi muscles to a line which is perpendicular to the body, and is quickly again depressed to the horizontal position by the pectorales. This constitutes the first stage of the 'stroke.' 'Recover' is initiated by an inward rotation of the humerus, semiflexion of the wing at the elbow (the pinion remaining extended and directed obliquely downward and outward), and is carried well forward to a degree sufficient, when seen in profile, to conceal the head. In this position the primaries are semirotated so as to present the least amount of surface to the air in the direction in which the bird is moving. The impetus excited by the stroke carries the bird upward and forward. In the second stage of 'recover,' the humerus is rotated outward, the arm is quickly raised, the primaries restored to the position seen in the bird at rest, and the wing is a second time in the position for the 'stroke.' In the eagle and the hawk the legs are in the position of the 'stroke' when the wings are similarly placed. During the 'stroke' the legs move backward. This motion continues during the 'recover' of the wing, so that the time of the 'recover' of the wing is also that of the 'recover' of the leg. The action of both wings and feet, since both pairs act together, is what I propose to call 'synadelphic.'

The study of the flight was confined to the eagle, the hawk, the pigeon, and the parrot, in the series of instantaneous photographs taken by Mr. Edward Muybridge, under the auspices of the University of Pennsylvania.

HARRISON ALLEN.

Philadelphia, March 7.

On the serpentine of Syracuse, N.Y.

An especial interest attaches to this rock for two reasons: 1°, because of the almost total absence of rocks of this class, or indeed of any intrusive rocks, from the undisturbed paleozoic strata of New York; and, 2°, because of the importance which has been recently attributed to it by Dr. T. Sterry Hunt, as affording evidence in favor of his chemical precipitation theory of the origin of serpentine.

The Syracuse serpentine was discovered in 1837, and was described by Vanuxem in his third annual report in 1839 (pp. 260 and 283), and in his final report on the geology of the third district in 1842 (p. 109). It is also mentioned by Beck, in his 'Mineralogy of New York,' as a 'dike or bed' (1842, p. 275). Dr. Hunt published an analysis of this rock in the *American journal of science* for 1858 (xxvi. p. 236), and has laid great stress upon it in his recent essay on the geological history of serpentines.

Through the courtesy of Prof. A. H. Chester of Hamilton college, the writer has been enabled to study a very complete suite of this rock and its associates, which was collected by the late Prof. Oren

Root while he was principal of the Syracuse academy. Mr. J. Forman Wilkinson of Syracuse, who was at this time one of Professor Root's pupils, has contributed several interesting points relating to the occurrence of the serpentine. In a recent letter to the writer, he says, in speaking of the different localities mentioned by Vanuxem and Beck, "The exact place was upon the lawn now owned and occupied by Howard G. White. . . . The specimens that you have were gathered some time between 1837 and 1845, probably nearer the earlier period. We used to go to the bed sometimes with a pick (oftener not) to gather and sort out the specimens. They were found in a bed of decomposed green rock, which was soft, and readily gave way under the pick. This bed of green disintegrated rock extended all along the side of the hill from the middle of James Street, nearly to the place where Howard White's house was built. The specimens were, I think, all found at the north or James Street end. . . . *When a trench was opened for water-mains opposite, and near to this deposit of serpentine (about fifty feet away), the cutting was through gypsum.*" The outcrop has not been accessible for over forty years.

It will be readily seen that the main point of interest connected with this rock is its mode of origin, — whether aqueous or igneous. It is included between two beds of porous limestone or dolomite. Among the dozen or more specimens in the possession of the writer, there are some which show angular fragments of this limestone embedded in the serpentine. In one case these are so abundant as to afford a breccia with a serpentine matrix. By far the best proof of the eruptive nature of the rock from which the serpentine has been derived is, however, afforded by its microscopic structure. The hand specimens agree exactly with the descriptions of Vanuxem and Beck. There are two principal varieties, — one a compact, dark-green rock, in which a few bronzy crystals are seen; and a mottled one, occasionally stained with blood-red spots. A microscopical examination shows that both of these rocks are most typical representatives of the class known as peridotites; the former with a slightly, the latter with a very pronounced, porphyritic structure. The original structure is still perfectly preserved, although most of the constituents are changed to serpentine or a carbonate. The groundmass contains, beside these two minerals, magnetite, a brown mica peculiarly characteristic of certain peridotites, green amphibole, and yellowish octahedrons which may prove to be anatase. The porphyritic crystals have the typical crystal forms of olivine or enstatite, both so perfect and so sharp that they could only be the early crystallizations from a fluid magma. The blood-red spots are seen to be due to the common staining of altered olivine crystals by iron hydroxide. The more porphyritic specimens are doubtless from the edge of the mass, and the coarser-grained variety from its centre.

The evidence of the eruptive origin of the Syracuse serpentine appears, therefore, to the writer to be: 1°. The microscopic structure, which shows that the original mineralogical composition and arrangement of the rock were such as are only found in masses of an eruptive nature; 2°. The included fragments of the adjacent limestone; 3°. The last remark quoted from Wilkinson's letter, that fifty feet away, on the strike of the deposit, only gypsum was encountered.

There seems to be nothing in any of the published descriptions of this deposit which indicates that its origin was aqueous. Such an idea, expressed by both Vanuxem and Hunt, is purely a matter of opinion, unsupported by any facts.

The writer hopes soon to publish in more detail the results of his study of this rock. It seems to bear a strong resemblance to the carboniferous peridotites recently described from Kentucky by Mr. J. S. Diller, of the U. S. geological survey, — an opinion with which Mr. Diller himself wholly concurs.

GEORGE H. WILLIAMS.

Baltimore, Md., March 7.

Thought-transference.

It is always a rash course to attack other people's work on the strength of second-hand reports of it, and doubly so when the reports have themselves been those of hostile critics. This rashness I am forced to impute to 'J. J.', the writer of a paper on 'Some miscalled cases of thought-transference,' in your supplement for Feb. 4, as I cannot for a moment believe him capable of the deliberate *suppressio veri* and *suggestio falsi* which his attempt to explain our English results by 'number-habits' would otherwise involve. The idea that the argument for thought-transference has depended entirely, or mainly, on experiments in which one person chose a number at will, and another person tried to guess it, could not survive the most cursory study of the published evidence. Yet that idea, picked up by 'J. J.' from an article in the *National review*, is the one on which his own criticism is expressly and exclusively founded, and which every one of his readers, if unacquainted with the original evidence or some trustworthy version of it, must at this moment be holding.

As a matter of fact, this type of experiment (though, as I shall show, 'J. J.' has greatly exaggerated its defects) has hardly ever been employed by us, and its results are a negligible quantity in our case. Our published records do not include a single instance in which the object to be guessed was a single digit chosen by the agent. Where the number contains two digits, the risk of appreciable disturbance of the results by 'number-habit' is of course far less; and trials of this type form between a sixth and a seventh part of the tabulated Creery aggregate.¹

But their importance in the cumulative result of those experiments is very much smaller than this fraction would indicate; since the success obtained in them, though very remarkable, was less so than in some other types. If 'J. J.' likes to omit them, one and all, as 'vitiated,' he is welcome to do so; and he will, at any rate, have the satisfaction of striking a certain number of noughts off the odds — estimated at about a hundred million trillions to 1 — against obtaining by accident the amount of success re-

¹ This aggregate consists of results where the object of which the idea was to be transferred was known only to some member or members of the investigating committee. See the table in 'Phantasms of the living,' vol. i, p. 25, as to which it should be noted, that in the experiments with single digits, included under the second head of Dublin experiments, the numbers were drawn at random out of a bag. Trials with "letters of the alphabet, and names of people and towns," by the way, find no place in this crucial list; but I am curious to know whether 'J. J.' would account, e.g., for the correspondences of names recorded on p. 27, by 'independent similar brain-functioning.'

corded. Our only other published instance of trials where double numbers were chosen, is that described in 'Phantasms of the living,' vol. i. p. 34; and here, as soon as we heard of certain remarkable results which were being obtained by two of our friends, we took the precaution (which 'J. J.' regards as beyond the capacity of such as us, though likely to occur to 'psychologists and writers on probabilities') of insisting that the numbers should be *drawn*, and not *chosen*, by the agent. This precaution has, of course, been invariable in our principal class of experiments, where the objects to be guessed have been playing-cards. Of two long series recorded in 'Phantasms' (vol. i. p. 34, and vol. ii. p. 654), where double numbers were similarly drawn, one gave as the total of completely correct guesses a result against the accidental occurrence of which the odds were over two millions to 1; the other, where account was taken of cases where the two right digits were guessed in reverse order, and of cases where one only of the digits was guessed rightly and in the right place, gives a total result against the accidental occurrence of which the odds were nearly two hundred thousand million trillion trillions to 1.

I have perhaps said enough to indicate the extent of 'J. J.'s' misrepresentation; but I may further briefly point out how defective his reasoning would be, even supposing that experiments of the sort attacked had really occupied the place in our evidence which he supposes. 1. His own remark, that the discovery of 'number-habit' was "brought about by noticing that quite constantly an undue number of successes occurred at the *beginning* of many sets of number-guessings," might have suggested to him how slightly it was likely to affect long series, where all the numbers appear again and again. To make out his case, he must get a few uninitiated persons each to write down a series of, say, fifty digits, and must ascertain by comparing the first, the second, the third items, and so on, of each pair of lists, whether the number of correspondences in each pair far exceeds the ten (one-tenth of the total), which is the theoretic most probable number, and, if so, how far such excess is connected with the predominance of one or two particular digits. How the correspondences could be produced by a *'varying'* predilection for *different* numbers, I must leave it to him, or the writers whom he quotes, to explain. 2. The cases he adduces where 'persons were asked to choose a number, *no limits being set*,' and then, as a rule, chose numbers under 20 or under 10, are quite irrelevant. We never, on any occasion, gave this unlimited choice, which would have precluded the knowledge of exactly what it was most essential to know,—the degree of probability that chance would produce the results obtained. 3. The fact that many people, when asked to choose a number with three figures, choose a number containing the digit 3, is quite irrelevant: for, in the first place, we have never experimented with numbers of three digits; and, in the second place, the fact that 3 sensibly predominates in a number of *first* choices does not even tend to suggest that it would sensibly predominate in a *series* of choices. 4. To experiments with double numbers (when chosen and not drawn), 'J. J.' objects that people are apt to choose multiples of ten with disproportionate frequency, and that they tend to choose numbers near the higher limit. A glance at the double-number results recorded in 'Phantasms of the living' (vol. i. p. 34)

will show the futility of making a serious objection to them out of the slight preference¹ for multiples of ten; for the number of successes (obtained before the plan of drawing from a bowl was introduced) exceeded what chance was likely to give, even supposing that the agent's choices and the percipient's guesses *had throughout been restricted* to multiples of ten—restricted, that is, to nine out of the ninety numbers over which they freely ranged. As regards the alleged predilection for later numbers, I need only remark that in a series of any length it ceases to be apparent;² while, even if it continued, the later numbers in a set of ninety are sufficiently numerous to insure, at each trial, large odds against accidental success.

In conclusion, I cordially agree with 'J. J.' in recommending (as my colleagues and I have recommended publicly and privately times without number) such forms of experiment as leave the issue between chance and thought-transference perfectly clear. I am also glad to find him, and the writers whom he quotes, so completely sound on another point which I have specially urged,—the fallacy of extracting evidence for thought-transference from the frequent simultaneous utterances of thought and feeling by relatives and intimate associates. Such fallacies cannot be too often exposed; for telepathy suffers far more from friends who accept and proclaim it on insufficient grounds than from its most strenuous critics and opponents. Whether 'J. J.' would continue to hold *our* grounds insufficient, if he took the trouble to learn what they are, I cannot tell; meanwhile he must pardon my feeling a certain sense of alliance with one who so clearly perceives that the novel doctrine, though evidence may prove it, could never be proved by casual experiments or by loose, popular arguments. How soon the proof will be generally recognized as complete, depends on something which we, unfortunately, can neither foresee nor control,—the degree in which sympathy with our objects and methods takes the form of help.

By chance, I have only just seen *Science* for Jan. 21, in which I read that Dr. Minot has lately introduced some trick-experiments with cards as similar to some of our thought-transference trials. In Dr. Minot's cases the card was forced on the drawer by a confederate of the professing 'percipient.' In all our card-experiments the card was drawn at random from the pack by one of our own investigating group. For these cases to resemble Dr. Minot's, it would be necessary that the percipient, or some one connected with the percipient, should have held the pack while the card was drawn. To permit such a procedure would have implied a degree of incompetence on our part which it did not occur to us explicitly to disclaim. However, I take this opportunity of disclaiming it, by stating that the pack was invariably held by one of ourselves; almost always, in fact, by the person who made the draw.

Dr. Minot is further reported to have objected that "in many of the English experiments there existed

¹ I have just examined the details of 1,191 of these trials, which I have under my hand, and find that the cases where multiples of ten were chosen form rather more than an eighth, instead of a ninth, of the whole.

² I have examined three hundreds, taken at random, of the series just mentioned. In the first hundred, 53 of the numbers chosen were nearer the higher limit than the lower; in the second and the third hundred, 55 were nearer the lower limit.

evident opportunities for fraud." Quite true—not in many only, but in all; and not only in psychical but in physical experiments of all sorts, which people accept without verifying the results for themselves. But *whose* fraud? We have always been content to rely on the very large class of cases in which the fraud would have had to be *our own*,—fraud in which the investigators actively shared, not merely which they failed to detect. I am far from saying that Dr. Minot or any one else is bound to accept this condition as crucial. But it is surely obvious that he who carries his experiments to the point where they can only be impugned by impugning his good faith, has done—as far as the *quality* of his results is concerned—all that any experimenter in any branch of science ever can do. Nothing remains, after this, but to try to increase the *quantity* of the results, whereby the responsibility for them may be spread over other shoulders.

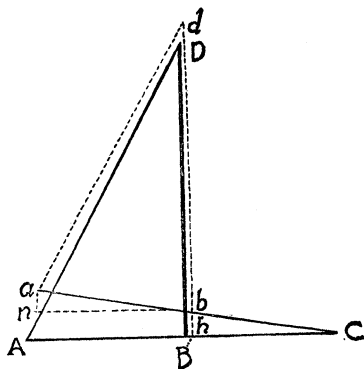
EDMUND GURNEY.

London, Feb. 17.

On tiptoe.

About two years ago Mr. F. A. Pond requested me to work out for him the problem of the human foot regarded as a lever. He thought the essential feature of the case—namely, the attachment of the calf-muscle to the leg below the knee, as well as to the heel, by a tendon—had been ignored.

The question has been of interest to a number of people; and it may be well to bring the true state of the case before writers on anatomy and physiology, inasmuch as it appears to be generally stated that the foot is a lever of the second order when used in rising 'on tiptoe.'



It will do to assume the change of position so small that the foot may be treated as a straight lever. Let $A B C$ be the foot-lever: A , the point of attachment of tendon to heel; B , the ankle pivot; and C , the point where the foot rests upon the ground. At B erect a perpendicular, BD , to represent the leg-bones, the calf-muscle being attached at D . Now let the muscle contract, and raise B to b . The work done is equal to the weight of the body (supposing one foot used) multiplied by the perpendicular distance through which B is raised, that is, bh of the figure. The power exerted by the muscle is equal to its pull multiplied by the diminution of the distance AD . As B rises to b , let A rise to a , and D to d . Through b draw bn parallel to AC , and drop an .

Now, bC is to bh as ba is to an . The line an is very approximately the amount of shortening of the muscle. The sign of the 'mechanical advantage' will be positive, zero, or negative, according as AB is greater than, equal to, or less than, BC . A lever of the 'second order' implies advantage of positive sign; that is, so-called 'mechanical advantage.' A lever of the 'third order' implies mechanical disadvantage. A lever of the 'first order' is capable of affording mechanical advantage or mechanical disadvantage, as the ratio of the arms determines: hence, when one rises on tiptoe, the foot is a lever of the first order.

An attempt has been made to regard the case as of the second order, by calling the upward pull at A , y , and the pressure of the body at B , x . The pull y will be transferred as a downward thrust of y to B ; so that we have (if, for instance, $AB = BC$) an upward force of y at A , and a downward force of $x + y$, equal to $2y$, at B . But the traverse of y is not twice the traverse of $2y$. Thus the 'principle of work' limits the case to the 'first order.'

F. C. VAN DYCK.

New Brunswick, N.J., Feb. 28.

Increase of the electrical potential of the atmosphere with elevation.

Very many observations of the electrical potential of the atmosphere have been made at different places in this country during the past year, under the auspices of the U.S. signal office. Among others, at Washington, D.C., a series of simultaneous observations has been carried on at the instrument room of the signal office and at the top of the Washington monument, the highest known edifice. The object of the present paper, published by permission of the chief signal officer, Gen. A. W. Greely, is to present in brief some of the results of those observations, particularly those bearing on the value of the intensity of the electrical force of the atmosphere at an elevation of five hundred feet, and the variations of the potential under different conditions of weather.

Beccaria, De Romas, Henley, and Cavallo, all noticed that the more elevated the position of the collecting apparatus, the greater the degree of electrification. Schübler (*Schweigg. Journ.* ix. 348) was the first to make measurements of the difference, and found that a positive electrification increased, at least up to a height of 50.5 metres. His results with an electroscope were as follows:—

Height (metres).....	9.7	16.2	24.4	47.1	49.4	55.6	58.5
Deflection (degrees).....	15	20	26	50	53	55	64

Sir William Thomson, it is sometimes stated, found an increase of from 200 to 300 volts for three metres. This value, however, was one obtained with a portable electrometer on a flat open sea-beach on the island of Arran, the height of the mast being nine feet above the earth. The readings varied from 200 to 400 volts, so that "the intensity of electric force, perpendicular to the earth's surface, must have amounted to from 22 to 44 Daniell elements per foot of air" (Thomson, reprint of papers, xvi. 281). It is also intimated that on other dates this value might have been twice as large, or yet much smaller. Mascart and Joubert found that if two water-collectors were placed in the same vertical line, the one five, the other ten metres high, the indications were in the main alike, and in the ratio of 1 to 2. Some experi-

ments made by me in May, 1886, confirm this general statement, although the actual values would vary greatly from day to day. Thus, with two collectors, on one date I obtained as mean values, for 80 feet elevation, 150 volts; for 55 feet elevation, 40 volts; while on another date the values for the same elevations were respectively 300 volts and 100 volts.

Professor Exner (*Repertorium der Physik*, xxii. heft 8, 451) gives the results of some experiments of a similar nature made about the same time, which show the potential gradient to be of uncertain value, and influenced largely by the proximity of buildings and walls. The following values for the potential were obtained with a water-dropper in an enclosed court:—

Two metres from wall	{Height (metres)..... 0 5 10 15 20 Potential (volts)..... 0 2 7 17 48
In centre of court.....	{Height (metres)..... 0 5 10 15 20 Potential (volts)..... 0 5 11 32 68

From measurements made with small balloons filled with hydrogen gas, Exner obtained, for the potential in free air, these values:—

Height (met.)	17 18 20 21 22 24 25 27 30 34 40 48
Poten. (volts)	100 110 {120 130 160 160 160 170 {195 250 280 350 140 210

from which

$$F = \frac{dV}{dn} = 6.8 \frac{\text{volt}}{\text{metre}}.$$

These values were obtained with a burning match. According to Pellat (*Comptes rendus*, c. 1885), the collecting efficiency of the match, compared with water-dropper and flame, is in the ratio of 1 to 5 to 10; so that, for comparison with the observations made here, where a water-dropping collector is employed, we have as a value for the electric force, during calm fine weather,

$$F = 34 \frac{\text{volt}}{\text{metre}}.$$

Another set of observations, made on an exposed mountain-side, gave these results:—

Height (metres)	3 5 6 7 12 14 18 19 20 25 30
Potential (volts)	110 {140 210 {230 330 {380 480 {520 550 660 820 970 150 250 405 550

or there is a linear potential gradient, but with a higher value than in the preceding experiments. Supposing a water-dropper to have been employed instead of flame as the collecting agency, we have the value

$$\frac{dV}{dn} = 159 \frac{\text{volt}}{\text{metre}}.$$

It is evident, then, that this value of the electrical force of the atmosphere is uncertain, and determined largely by local surroundings. It is also further affected by the conditions of temperature and relative humidity, and, as intimated, by inconstancy of the collecting agency. In working toward that 'electrogeodesy' which Sir William Thomson has proposed, we must determine and allow for these and doubtless other influences. By taking the mean of many observations made at different times, the influences of temperature and humidity are to some extent avoided. As said above, the following observations were made simultaneously, in 1886-87, at the top of the monument, 500 feet above the ground, and at the signal office, at an elevation of 50 feet. The instruments used were modified Mascart electrometers, and large water-droppers with nozzles of the same size. Similar methods and adjunct apparatus were employed at both places. The values in the follow-

ing table appear to be too small, judging from the results quoted above. But it is to be remembered that these observations are made in both cases from buildings, and the points in air at which the collecting stream breaks away are not very distant from the side of the building.

Values of electric force of the atmosphere.

Date.	Number of observations.	Mean value of potential.		Difference for 450 feet.
		Monument.	Signal office.	
		Volts.	Volts.	Volts.
June 26	399 consecutive 5-minute observations	289	134	155
" 27				
" 28				
July 17	60	1129	93	1036
" 20	107	389	70	319
Sept. 21	40	212	107	105
Oct. 4	94	586	192	394
" 5	82	300	108	192
" 7	97	435	112	323
" 14	87	140	24 (a)	116 (a)
Nov. 1	4 (b)	1137	265	872
" 3	98	943	248	695
" 12	15	-849 (c)	-245 (c)	-604 (c)
" 12	65	458	36	422
Dec. 15	13	487	4 (d)	483 (d)
Jan. 29	26	413	141	272
Feb. 9	54	1825	89	1736

(a) On this date some of the values at the lower station were below the zero, i.e., negative: 69 observations gave positive indications, averaging 38 volts, and 18 observations gave negative values, averaging 31 volts. The negative values have been subtracted from the positive, and the remainder divided by the total number of observations.

(b) Not simultaneous.

(c) At both stations during rain the observations continued for some little while negative.

(d) As in (a).

We have, therefore, from the above table, a mean value of the potential for the top of the monument of 637 volts, and a value of

$$\frac{dV}{dn} = 4.33 \frac{\text{volt}}{\text{metre}};$$

and at the lower station a mean value of the potential of 124 volts and a value of

$$\frac{dV}{dn} = 8.43 \frac{\text{volt}}{\text{metre}}.$$

Therefore it would seem that the mean value of the potential at the upper station is about five times that at the lower station. Among the observations, I find one striking confirmation of this ratio. On Nov. 3, 1886, if we multiply the results obtained at the lower station by 5, we shall obtain approximately a duplicate of those at the upper elevation; this for a series extending from 11 A.M. until 3 P.M. In some respects this date was most satisfactory, being a dry, somewhat hazy, autumn day, with light southerly winds, and sky about half covered with ill-defined cirro-stratus clouds. The electrification at the top of the monument was sufficient to give a spark a millimetre in length.

These experiments were begun under the direction of Prof. T. C. Mendenhall, to whom, and to Col. T. L. Casey, of the Engineer corps, U.S.A., more than acknowledgment of kindness is due.

ALEXANDER MCADIE, M.A.

SCIENCE.—SUPPLEMENT.

FRIDAY, MARCH 11, 1887.

THE CHARACTERISTIC CURVES OF COMPOSITION.

AUGUSTUS DEMORGAN somewhere remarks (I think it is in his 'Budget of paradoxes') that some time somebody will institute a comparison among writers in regard to the average length of

mean word-length suggested itself. The new method, while scarcely more laborious than that proposed by DeMorgan, promised to yield results more quickly and of a definitely higher order. It also had the advantage of including, in its application, all that was necessary to the determination of mean word-length; so that, in reality, it furnished two distinct tests.

Preliminary trials of the method have furnished

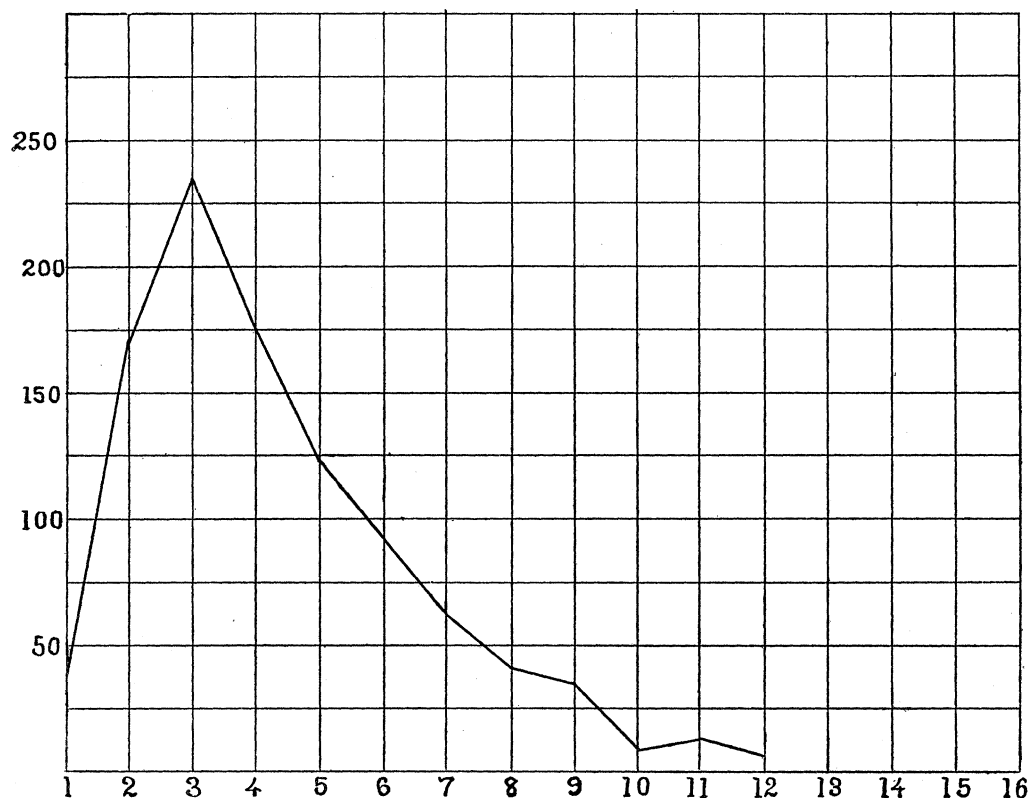


FIG. 1. — FIRST ONE THOUSAND WORDS IN 'OLIVER TWIST.'

words used in composition, and that it may be found possible to identify the author of a book, a poem, or a play, in this way.

In reflecting upon this remark at various times within the past five or six years, always with the determination to test the value of the suggestion whenever time for the work seemed available, a more comprehensive and satisfactory method of analysis than that based simply upon

strong grounds for the belief that it may prove useful as a method of analysis leading to identification or discrimination of authorship, and it is therefore brought to the attention of the scientific and literary public in the hope that some one may be found who is at once able and willing to secure a satisfactory test of its validity.

The nature of the process is extremely simple, but it may be useful to point out its similarity to

a well-known method of material analysis, the consideration of which actually first suggested to the writer its literary analogue.

By the use of the spectroscope, a beam of non-homogeneous light is analyzed, and its components assorted according to their wave-length. As is well known, each element, when intensely heated under proper conditions, sends forth light which, upon prismatic analysis, is found to consist of groups of waves of definite length, and appearing

every author, as with every element, this spectrum persists in its form and appearance, the value of the method will be at once conceded. It has been proved that the spectrum of hydrogen is the same, whether that element is obtained from the water of the ocean or from the vapor of the atmosphere. Wherever and whenever it appears, it means hydrogen. If it can be proved that the word-spectrum or characteristic curve exhibited by an analysis of 'David Copperfield'

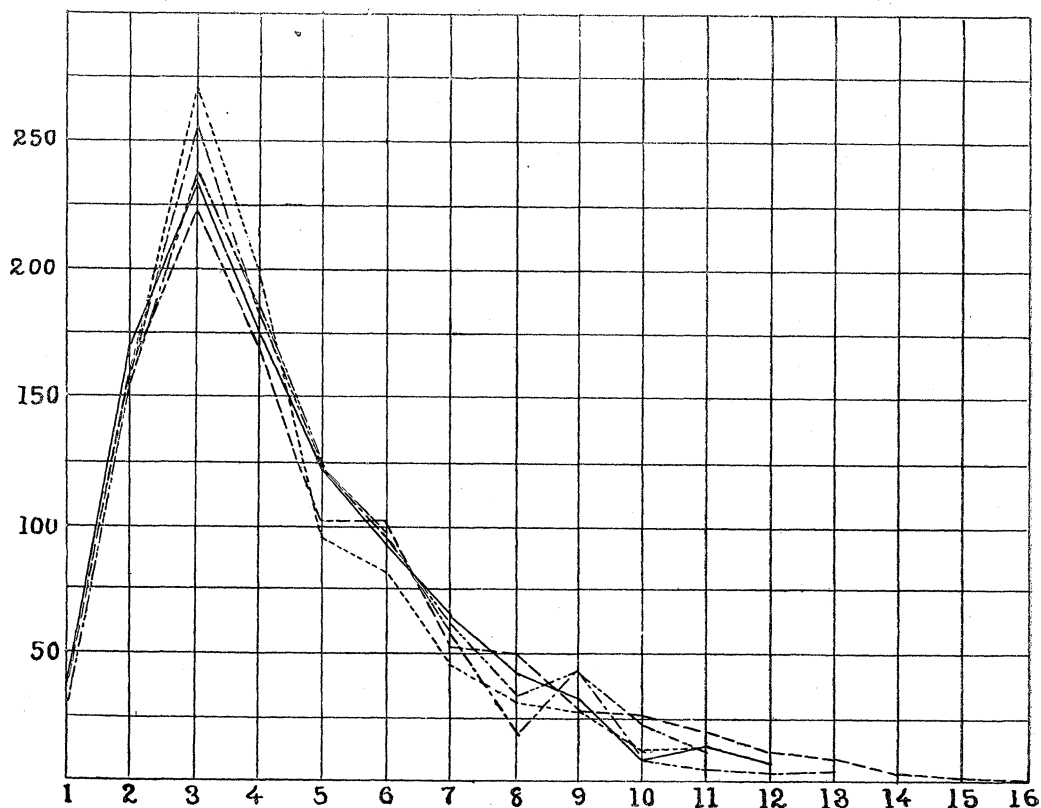


FIG. 2.—SHOWING FIVE GROUPS, OF ONE THOUSAND WORDS EACH, FROM 'OLIVER TWIST.'

in certain definite proportions. So certain and uniform are the results of this analysis, that the appearance of a particular spectrum is indisputable evidence of the presence of the element to which it belongs.

In a manner very similar, it is proposed to analyze a composition by forming what may be called a 'word-spectrum,' or 'characteristic curve,' which shall be a graphic representation of an arrangement of words according to their length and to the relative frequency of their occurrence. If, now, it shall be found that with

is identical with that of 'Oliver Twist,' of 'Barnaby Rudge,' of 'Great expectations,' of the 'Child's history of England,' etc., and that it differs sensibly from that of 'Vanity fair,' or 'Eugene Aram,' or 'Robinson Crusoe,' or 'Don Quixote,' or any thing else in fact, then the conclusion will be tolerably certain that when it appears it means Dickens.

The validity of the method as a test of authorship, then, implies the following assumptions: that every writer makes use of a vocabulary which is peculiar to himself, and the character of

which does not materially change from year to year during his productive period; that, in the use of that vocabulary in composition, personal peculiarities in the construction of sentences will, *in the long-run*, recur with such regularity that short words, long words, and words of medium length, will occur with definite relative frequencies.

The first assumption will, perhaps, be admitted in a general way, without debate. It is easily

in their curves, and consequently as a severe test of the method, two contemporaneous novelists, Dickens and Thackeray, were selected for the first examination. The operation consisted simply in counting the number of letters in every word, and recording the number of words of one letter, two letters, three letters, etc. The count began in both cases at the beginning of the volume, and, after a few thousand words had been counted in order, the book was opened at random near the

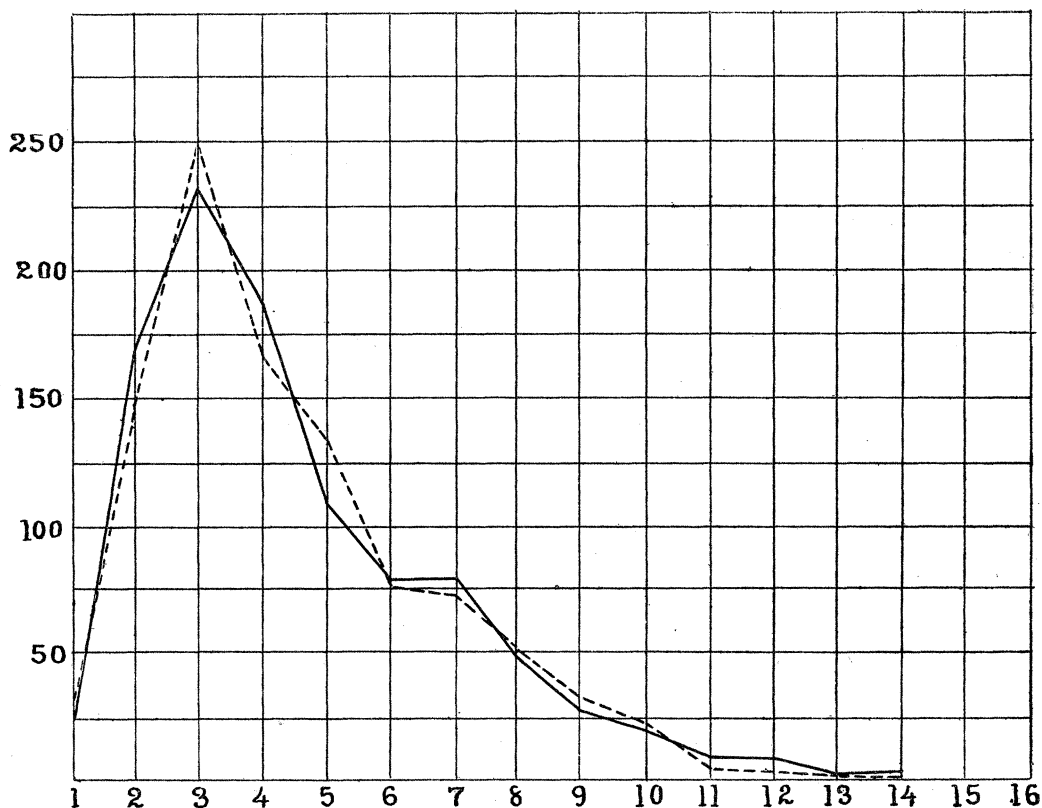


FIG. 3.—TWO CONSECUTIVE GROUPS, OF ONE THOUSAND WORDS EACH, FROM 'VANITY FAIR.' THESE GROUPS SHOW SENSIBLY THE SAME AVERAGE WORD-LENGTHS.

seen that to prove or disprove the second will require the expenditure of an enormous amount of labor. The following results are offered as a means of properly exhibiting the method, and as evidence, in some degree at least, of its real value.

It is important, first, to determine to what extent an author may be said to agree with himself; and, second, to what extent does he differ from others.

As an instance in which two writers might well be expected to greatly resemble each other

middle, and the count continued. In no case was any personal choice exercised, except that both counts began with the first chapter. Words were counted always in groups of one thousand. The graphic display of the result was made by the common method of rectangular co-ordinates, using the number of letters in a word as an abscissa, and the corresponding number of such words in a thousand as an ordinate. As an illustration, the first one thousand words counted from 'Oliver Twist' may be cited; they were as follows:—

Number of letters	1	2	3	4	5	6	7	8	9	10	11	12
Number of words	38	170	235	175	123	91	62	41	35	10	13	7

Even in so small a number as one thousand, the relative distribution of words is approximately the same as in a much larger number, although, as would naturally be expected, accidental variations or 'runs' overshadow personal characteris-

placing the numbers showing letters in each word at points along a horizontal line separated from each other by equal distances, above each of these place other points whose distance from the base line shall be proportional to the number of such words in a thousand; then join these points by a broken line, and the characteristic curve is shown. Fig. 1 shows the curve thus constructed from the first thousand words in 'Oliver Twist,' the numerical analysis of which is shown above.

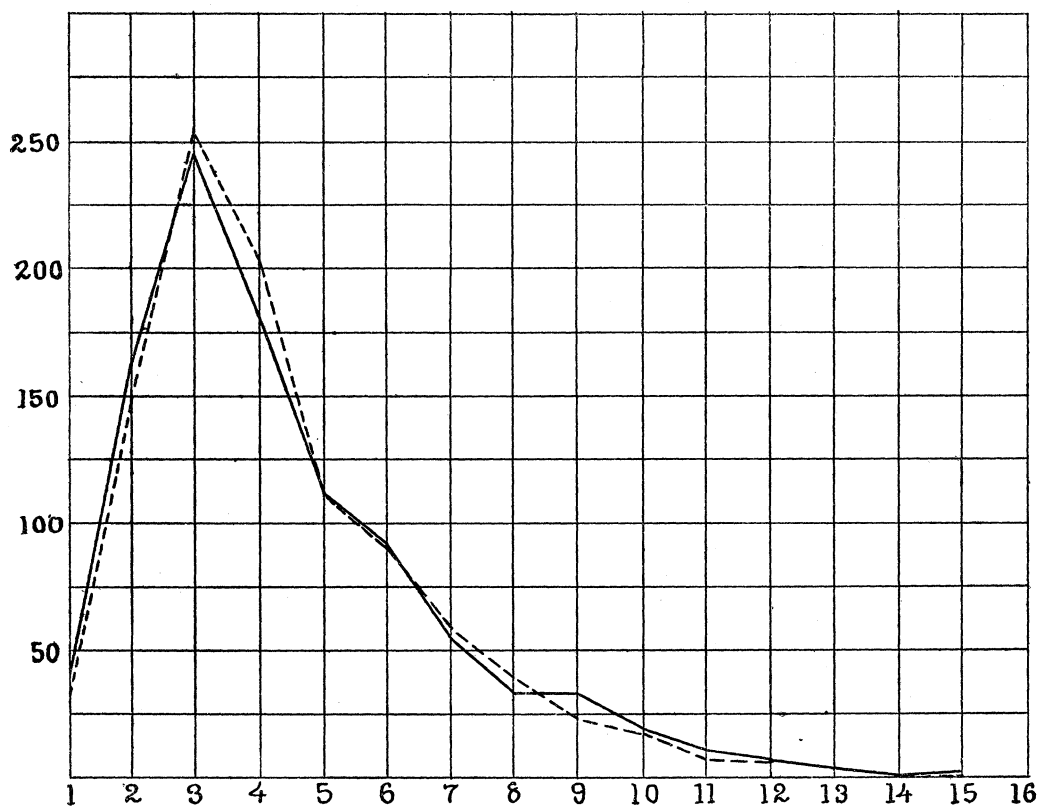


FIG. 4.—TWO GROUPS, OF FIVE THOUSAND WORDS EACH, FROM 'OLIVER TWIST.'

tics to a great extent; but not completely, as will be seen in the characteristic curves shown in the following pages. In fact, when the ten groups, of a thousand words each, from Dickens, are compared with ten similar groups from John Stuart Mill, no one of the first set could by any possibility be mistaken for any one of the second.

The graphic representation of the results will be readily understood. It is only necessary to take a sheet of 'squared' paper, or paper ruled in two directions at right angles to each other, and, after

The next diagram (fig. 2) exhibits five curves constructed from the first five thousand words the same from work, in groups of one thousand each. It is presented in order to show the variation among groups based on a relatively small number of words.

The superiority of this method over that of simple word averages, as suggested by DeMorgan, is clearly shown in fig. 3, which exhibits two consecutive groups, of one thousand words each, from 'Vanity fair.' The numerical analysis of these groups is as follows:—

Letters.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Words in 1st group	25	169	232	187	109	78	79	48	28	20	10	10	2	3
Words in 2d group	33	146	248	164	135	76	73	52	35	23	6	5	2	2

It will be seen that the total number of letters in the first group is 4,507, and in the second 4,508, or an average of 4.507 and 4.508 letters to each word in the respective groups. If this average,

ist. One of the curves shows an excess of nine-letter words, which does not appear in the other. They agree in showing a greater number of six-letter words than a smooth curve would demand. This excess may persist, and prove to be a real characteristic of Dickens's composition. Fig. 5 exhibits these two groups of five thousand words combined in one of ten thousand, giving a curve of greater smoothness, and approximating still more closely to the normal curve of the writer.

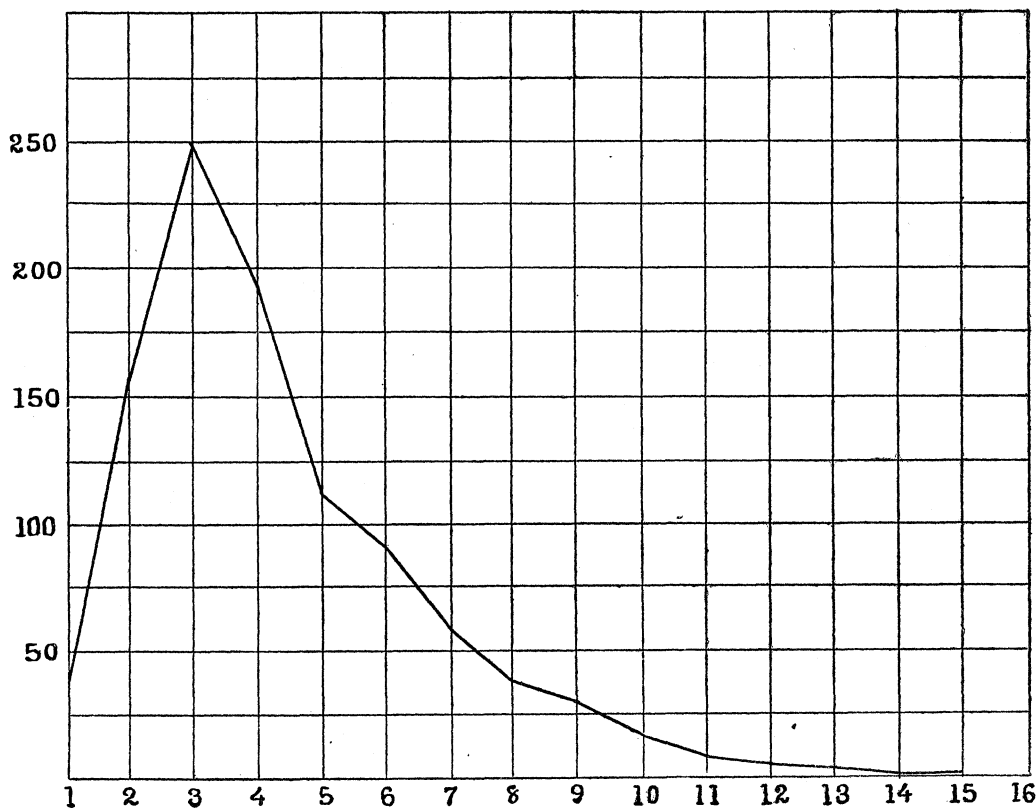


FIG. 5.—CURVE FOR TEN THOUSAND WORDS FROM 'OLIVER TWIST.'

or 'mean word-length,' be alone considered, the two groups must be regarded as sensibly identical; but an inspection of the diagram shows that they are in reality quite different.

When the number of words in a group is increased to five thousand, the accidental irregularities begin to disappear, the curve becomes smoother, approximating more nearly to the normal curve which, it is assumed, is characteristic of the writer. Fig. 4 exhibits two groups, each of five thousand words, from 'Oliver Twist,' and it will be seen that considerable differences still ex-

In fig. 6, two groups of five thousand words each, from 'Vanity fair,' are shown; and in fig. 7, two groups of ten thousand each, from 'Oliver Twist' and 'Vanity fair,' are placed side by side for comparison, the former being represented by the continuous line, and the latter by the broken line. Although these curves differ, and while it is believed that the difference will persist with an increased number of words, it is certainly surprising, that in the analysis of ten thousand words from Dickens, and the same number from Thackeray, so close an agreement

should be found. This agreement is particularly striking in words of eleven, twelve, and thirteen letters, the numerical comparison of which is as follows:—

Number of letters.....	11	12	13
Number of words in Dickens.....	85	57	29
Number of words in Thackeray....	85	53	29

ists; but I confess to considerable surprise on finding from the very beginning, that although, on the whole, this anticipation was realized, the word which occurred most frequently was not the three-letter word, as with both Dickens and Thackeray, but the word of two letters. Indeed, the word of two letters was not only relatively more frequent, but absolutely; that is to say, it occurred more frequently in the composition of Mill than in that of either of the novelists, and with great uniform-

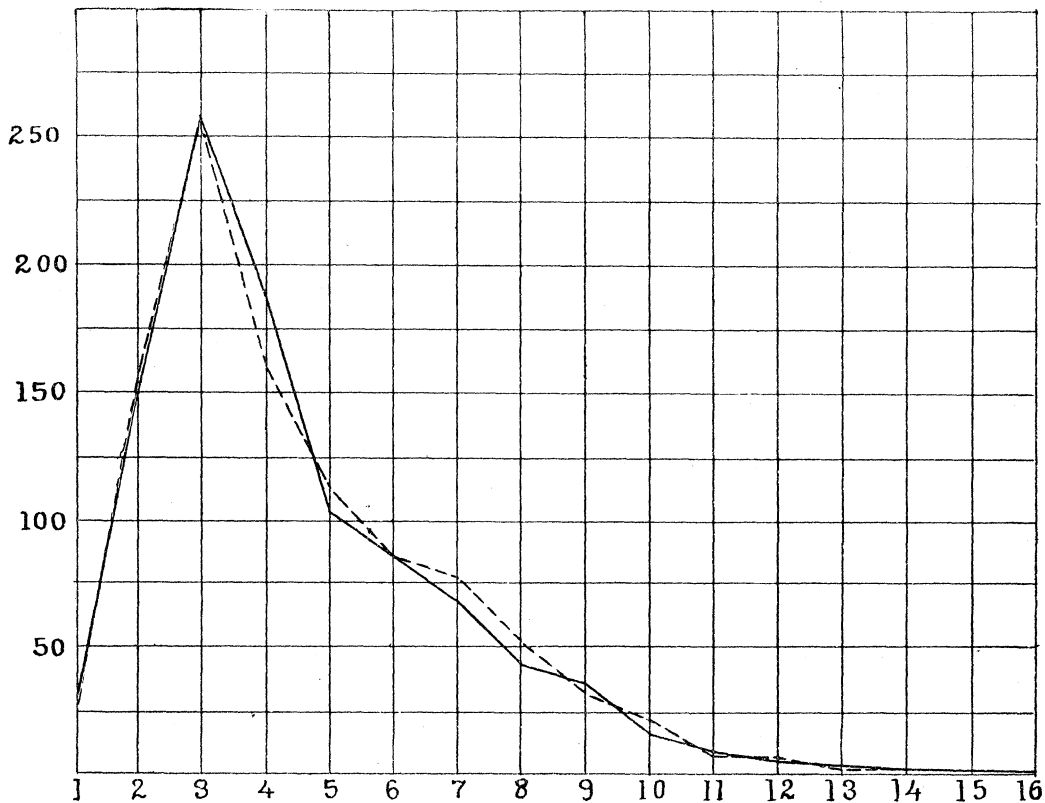


FIG. 6.—TWO GROUPS, OF FIVE THOUSAND WORDS EACH, FROM 'VANITY FAIR.'

This closeness to identity must be largely the result of accident, and it would not be likely to repeat itself in another analysis.

The writer next examined was John Stuart Mill; and to test the persistence of form in compositions belonging to different periods of the author's life, and upon different subjects, two groups of five thousand words each were taken,—one from his 'Political economy,' and the other from his 'Essay on liberty.' It was anticipated, of course, that words of greater length would occur far more frequently than in the case of the novel-

ity, as it was in excess in each thousand of the ten analyzed. The explanation is easy, and is to be found in the liberal use of prepositions in sentence-building. The proposed method of analysis is designed to reveal any peculiarity of this kind, and the exemplification of its power thus early in the work was encouraging.

Figs. 8 and 9 show the curves for five thousand words from the 'Political economy' and from the 'Essay on liberty.' It will be observed, that, while they differ considerably, there is still, in a general way, a striking resemblance, and that

they are in marked contrast with the curves of the novelists. An interesting case was furnished in two recent addresses on the labor question by Mr. Edward Atkinson. In reality, one address was given to two very different audiences. One was made up from the workingmen of Providence, and the other from the alumni of the Andover theological seminary. On reading the two, one cannot avoid being struck by the marked difference in style, although the two papers are much

The average length of ten thousand words in his addresses on the labor question is 4.298 letters. The mean word-length of the writers thus far examined, based upon a count of ten thousand words from each, is as follows:—

Atkinson.....	4.298
Dickens.....	4.342
Thackeray.....	4.481
Mill.....	4.775

A friend has furnished me with the result of the count of the first five thousand five hundred

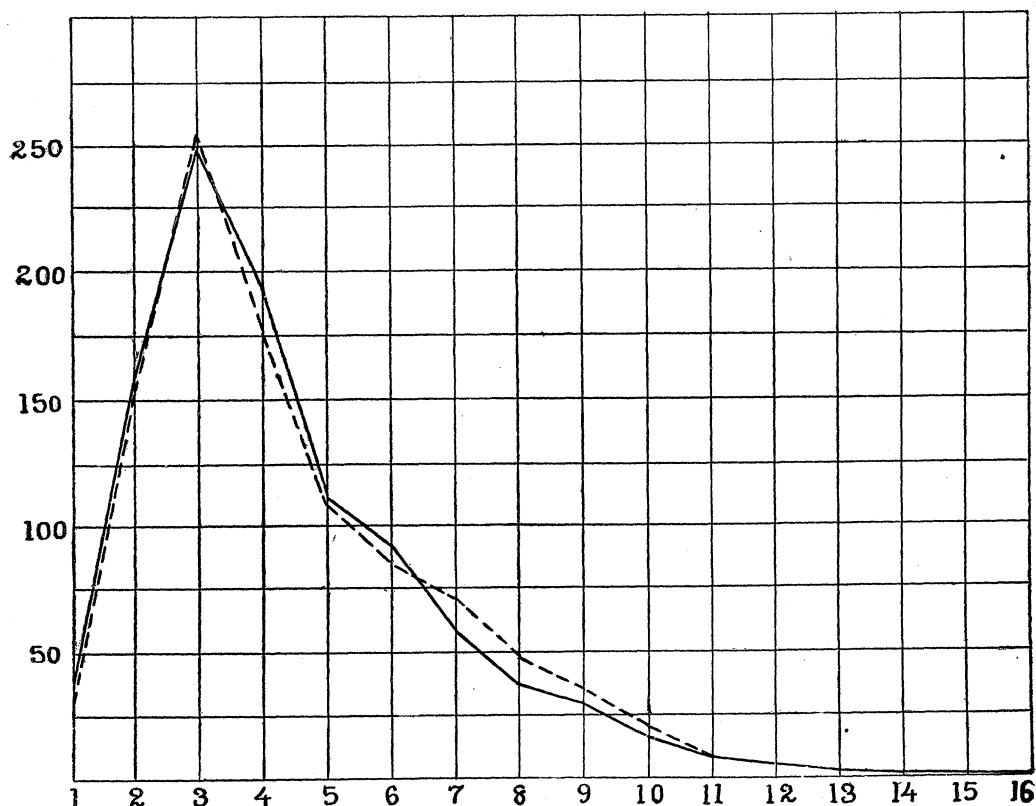


FIG. 7.—TWO GROUPS, OF TEN THOUSAND WORDS EACH, FROM 'OLIVER TWIST,'——; AND FROM 'VANITY FAIR,'-----.

alike in substance. It was interesting, then, to inquire whether their curves of composition would show any marked resemblance. An analysis of five thousand words from each paper was made, and the result is shown in fig. 10. A very satisfactory, indeed a striking, general resemblance will be observed; and it will also be seen that Mr. Atkinson's curve differs decidedly from others previously figured and described. It is shown in contrast with that of John Stuart Mill in fig. 11. Mr. Atkinson's composition is remarkable in respect to the shortness of the words used.

words of Caesar's 'Commentaries.' The mean word-length is 6.065. The most extensive word-counting that I know of is that of the words and letters in the Bible. I cannot vouch for the reliability of the information which periodically floats through the columns of the public press, that the Old Testament contains 592,493 words with 2,728,100 letters, and the New Testament 181,253 words with 838,380 letters. It is interesting to note, however, that these numbers give averages of 4.604 and 4.625 respectively, agreeing within less than one-half of one per cent.

Before making an analysis of Mr. Atkinson's composition, and after having counted more than thirty thousand from other writers, I had concluded that a group of one thousand words whose average length was less than four letters would not occur, except in compositions especially written in short words. Out of ten such groups from Mr. Atkinson's addresses, however, one was found whose mean word-length was 3.991. I have recently received from him a brief paper, entitled

method of analysis and identification has been furnished by several friends who have had the patience to enumerate the letters in many thousand words from different sources. Prof. Stanley Coulter sends me the result of a count of ten thousand from Dickens's 'Christmas carol.' He writes, "I became exceedingly interested in watching how little tricks of composition affected the 'curve.' For instance, one of the characters, 'Scrooge,' appears in one place very often, and an

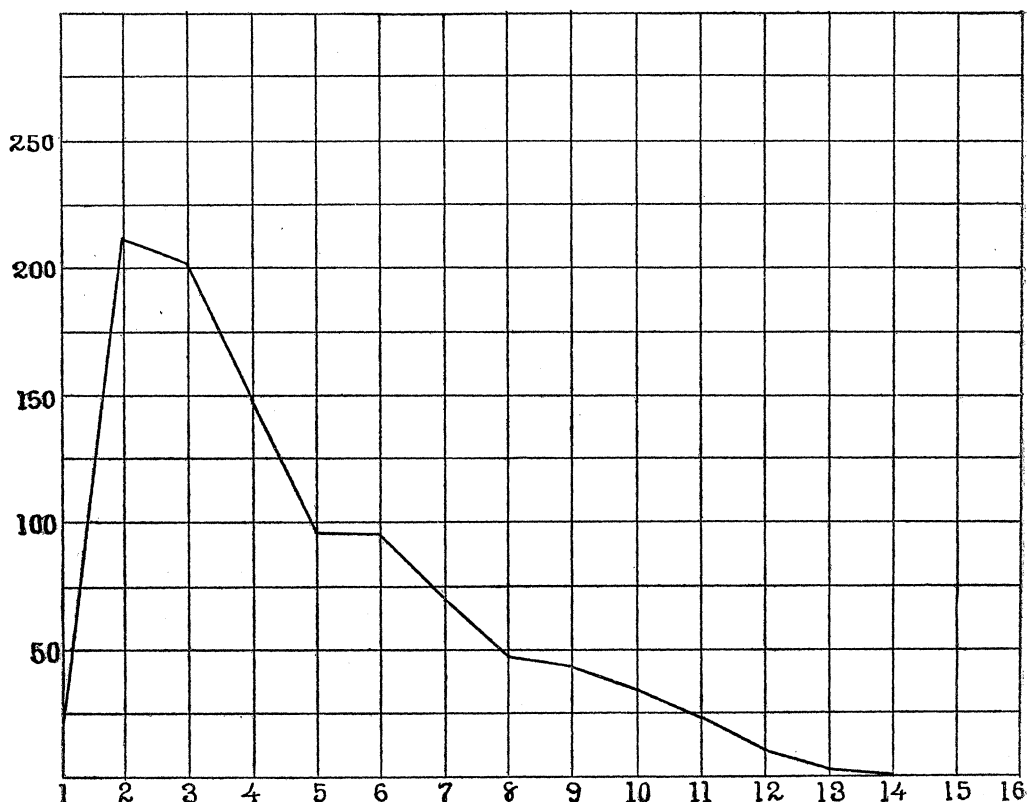


FIG. 8. — CURVE OF FIVE THOUSAND WORDS FROM MILL'S 'POLITICAL ECONOMY.'

'How do we all get a living?' which was published in *Work and wages*, and in the preparation of which he made a special effort to use the simplest language possible. The article contains a little more than two thousand words, the number being too small for the construction of a curve which would be comparable with those already exhibited. The general form of one based upon two thousand words is similar to that previously obtained from the same writer, and the mean word-length is 3.771.

Interesting evidence of the validity of this

excess of 7's is the result; in another place 'Fiz-ziwig,' and the 8's creep up [this is doubtless owing to the frequent appearance of the names]. Other variations and excesses seem to come from Dickens's love of certain forms of description, which he iterates and reiterates upon a single page."

I have plotted these ten thousand words from the 'Carol' with the ten thousand already shown from 'Oliver Twist,' in fig. 12. A very close resemblance will be observed, and it will be noticed that the *mean* of these two curves would be free from certain irregularities which occur in both,

and would be a much closer approximation to the normal characteristic curve of Dickens.

It is hardly necessary to say that the method is not necessarily confined to the analysis of a composition by means of its mean word-length: it may equally well be applied to the study of syllables, of words in sentences, and in various other ways. The results thus far obtained from its application would appear to justify the claim that it is worthy of a thorough test through which the

Many interesting applications of the process will suggest themselves to every reader; the most notable, of course, being the attempt to solve questions of disputed authorship, such as exist in reference to the letters of Junius, the plays of Shakspeare, and other less widely known examples. It might also be utilized in comparative language studies, in tracing the growth of a language, in studying the growth of the vocabulary from childhood to manhood, and in other direc-

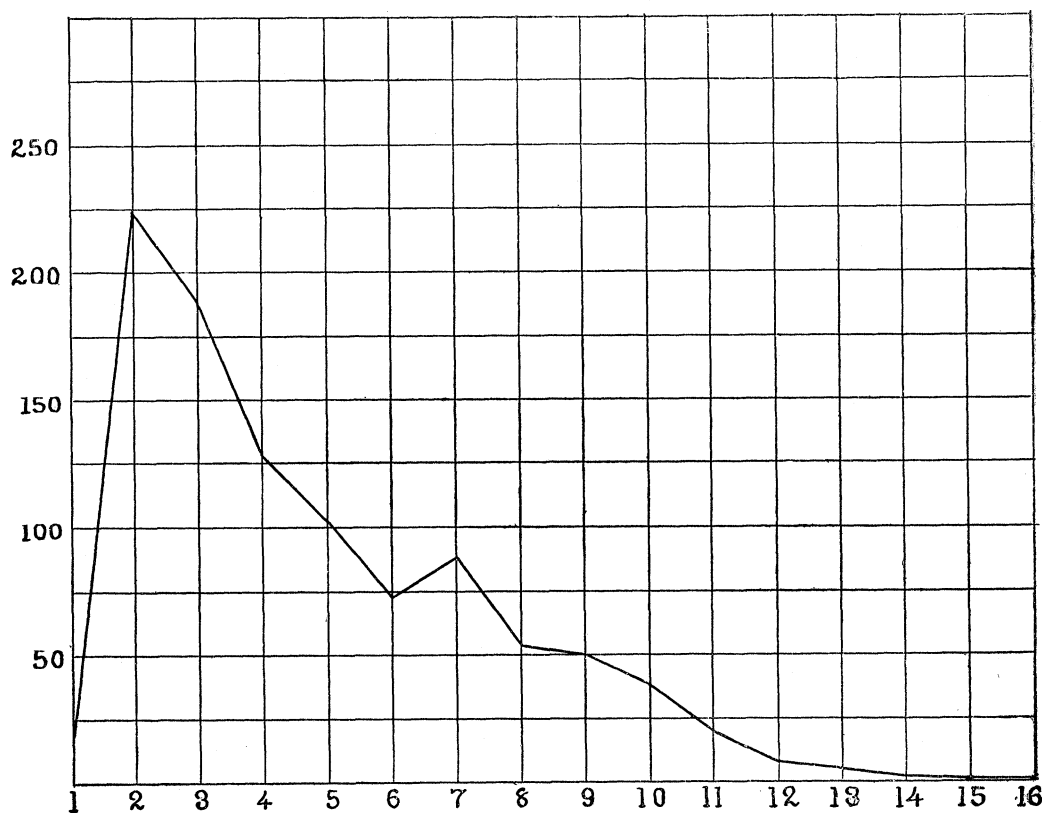


FIG. 9.—CURVE OF FIVE THOUSAND WORDS FROM MILL'S 'ESSAY ON LIBERTY.'

validity of its assumptions might be proved or disproved. Its principal merits are, that it offers a means of investigating and displaying the mere mechanism of composition, and that it is purely mechanical in its application. In virtue of the first, it might reveal characteristics which a writer would make no attempt to conceal, being himself unaware of their existence; and, of the second, the conclusions reached through its use would be independent of personal bias, the work of one person in the study of an author being at once comparable with that of any other.

tions too numerous to be catalogued. An illustration of its application to another language is shown in the analysis of more than five thousand words in Caesar's 'Commentaries,' already referred to, which is represented in fig. 13. The curve shows a relatively large use of long words, and its peculiar feature is the evident indication of two maximum ordinates nearly equal to each other.

From the examinations thus far made, I am convinced that one hundred thousand words will be necessary and sufficient to furnish the charac-

teristic curve of a writer,—that is to say, if a curve is constructed from one hundred thousand words of a writer, taken from any one of his productions, then a second curve constructed from another hundred thousand words would be practically identical with the first,—and that this curve would, in general, differ from that formed in the same way from the composition of another writer, to such an extent that one could always be distinguished from the other. To demonstrate the

though not probable, that two writers might show identical characteristic curves.

T. C. MENDENHALL.

TIDAL OBSERVATIONS OF THE GREELY EXPEDITION.

THE principal tidal observations were made at Fort Conger, on Lady Franklin Bay, by various members of the expeditionary force working under

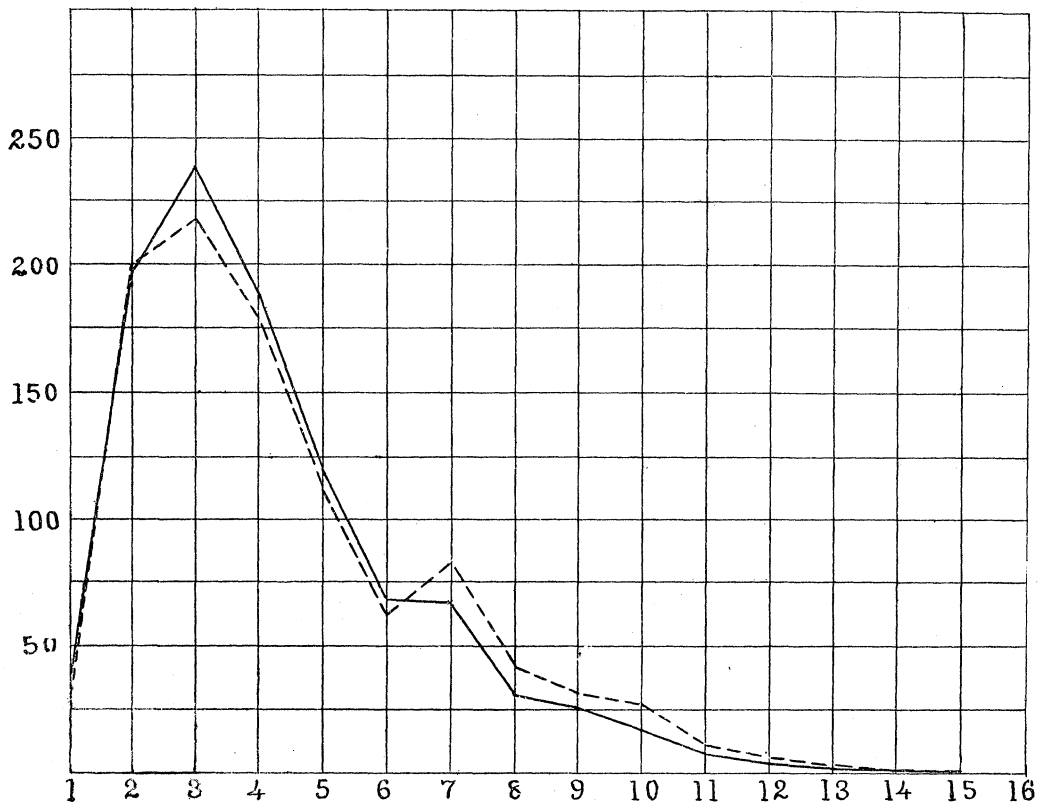


FIG. 10.—TWO GROUPS, OF FIVE THOUSAND WORDS EACH, FROM ADDRESSES OF EDWARD ATKINSON: ADDRESS TO WORKINGMEN, ———; TO ALUMNI OF THEOLOGICAL SEMINARY, — — —.

existence of such a curve will require the enumeration of the letters in several hundred thousand words from each of a number of writers. Should its existence be established, the method might then be applied to cases of disputed authorship. If striking differences are found between the curves of known and suspected compositions of any writer, the evidence against identity of authorship would be quite conclusive. If the two compositions should produce curves which are practically identical, the proof of a common origin would be less convincing; for it is possible, al-

though not probable, that two writers might show identical characteristic curves. They consisted of hourly heights of the tide from Aug. 20, 1881, to July 1, 1882, and the times and heights of high and low waters from Aug. 20, 1881, to June 30, 1883, both series read from fixed staff gauges and practically continuous. A broken series of high and low waters from July 1 to Aug. 8, 1883, obtained under unfavorable conditions, were not used in the discussion. There were also short series at seven outlying stations on the coasts of Greenland and

Grinnell Land, and a casual observation of high water at the head of Greely Fiord, during the progress of the readings at Fort Conger, with a dozen or more high and low waters noted during the retreat through Kennedy Channel and Kane Basin. The original records, too bulky for easy transportation, were left stored at Fort Conger when the party abandoned that station; but close transcripts, previously prepared and carefully verified, were brought away by Lieutenant Greely,

at stations beset with heavy ice, even short series are, as a rule, sadly out of joint and comparatively worthless. Unless the stability of the gauge is absolutely assured, which can seldom be the case, only frequent resort to levels between the gauge and one or more permanent bench-marks on shore can insure scientific value to the observations. At Fort Conger the observations of the first year depended in this respect upon a gauge that seems to have been stable, those of the second upon

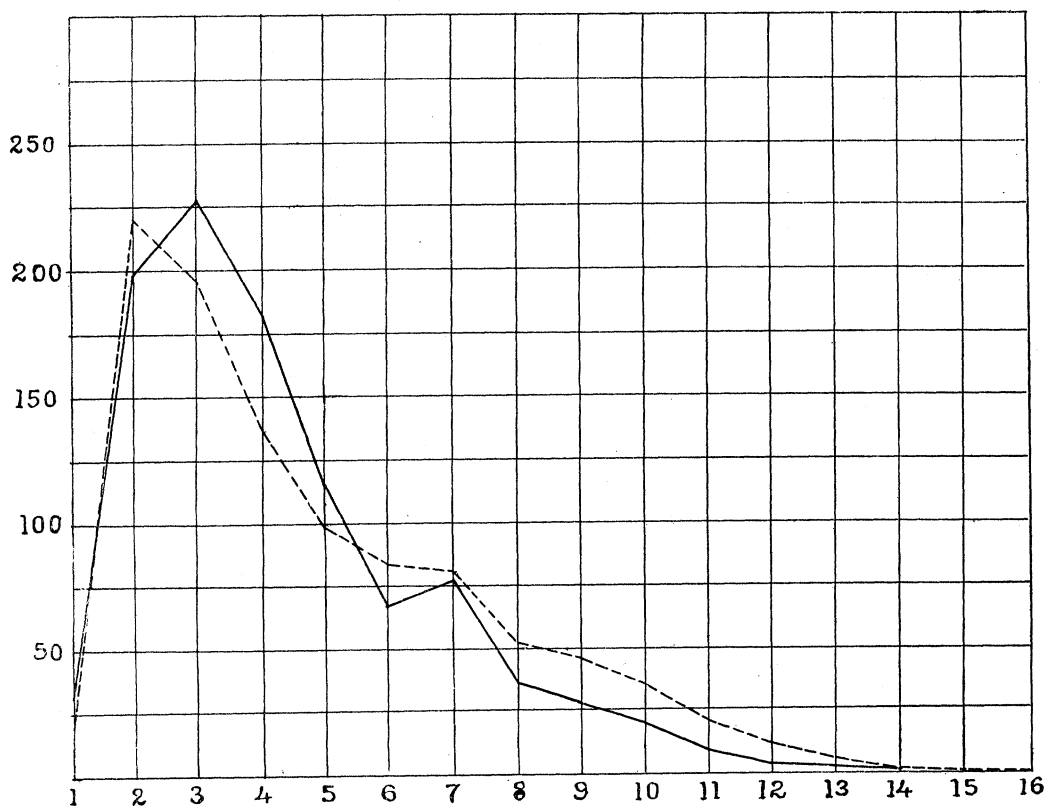


FIG. 11. — TWO GROUPS, OF TEN THOUSAND WORDS EACH. ATKINSON, ———; MILL, - - - -.

and on his return to this country referred to the superintendent of the coast and geodetic survey, and later were placed in the hands of Mr. Alex. S. Christie, chief of the tidal division of the office, for reduction and discussion.

The weak point of tidal observations is almost invariably, even in middle latitudes, the instability of the staff and the undetermined fluctuations in altitude of the staff zero; so that it not infrequently happens that a satisfactory reduction of all the observations to the same plane of reference is a wholly intractable problem. In high latitudes,

series of spirit-levels. Only two months of the series were in much doubt, and a tolerably satisfactory adjustment of these was finally effected. The observations bear abundant internal evidence of a conscientious and persistent endeavor to secure trustworthy and precise results; and, although they are far from equalling observations of standard excellence in middle latitudes, they are believed to constitute the longest and best series ever brought back from the arctic seas.

Following are some of the results of a non-harmonic analysis of the observations at Fort Conger:

the mean lunital intervals are $11^h 33^m.3$ and $17^h 45^m.3$; the mean range is 1.328 metres; the semi-mensual curves for intervals and heights give the age of the tide 1.4 days, the moon tide 2.2 times the sun tide, and satisfy closely the equilibrium formulæ of Bernoulli. The diurnal inequality in height is, in comparison with the whole tide, three times as small as in Smith Sound; the influence of the sun in producing it is practically equal to that of the moon; it van-

non of diurnal inequality is then taken up as a problem in kinematics, the diurnal inequality wave is analyzed into its principal components, and the sidereal period shown to have place at still other stations both within and without the arctic circle, and to be a rule rather than an exception. The results of an harmonic analysis of the first year's observations will be found in the report: in so far as they relate to the same matters, they confirm the results previously found and stated above.

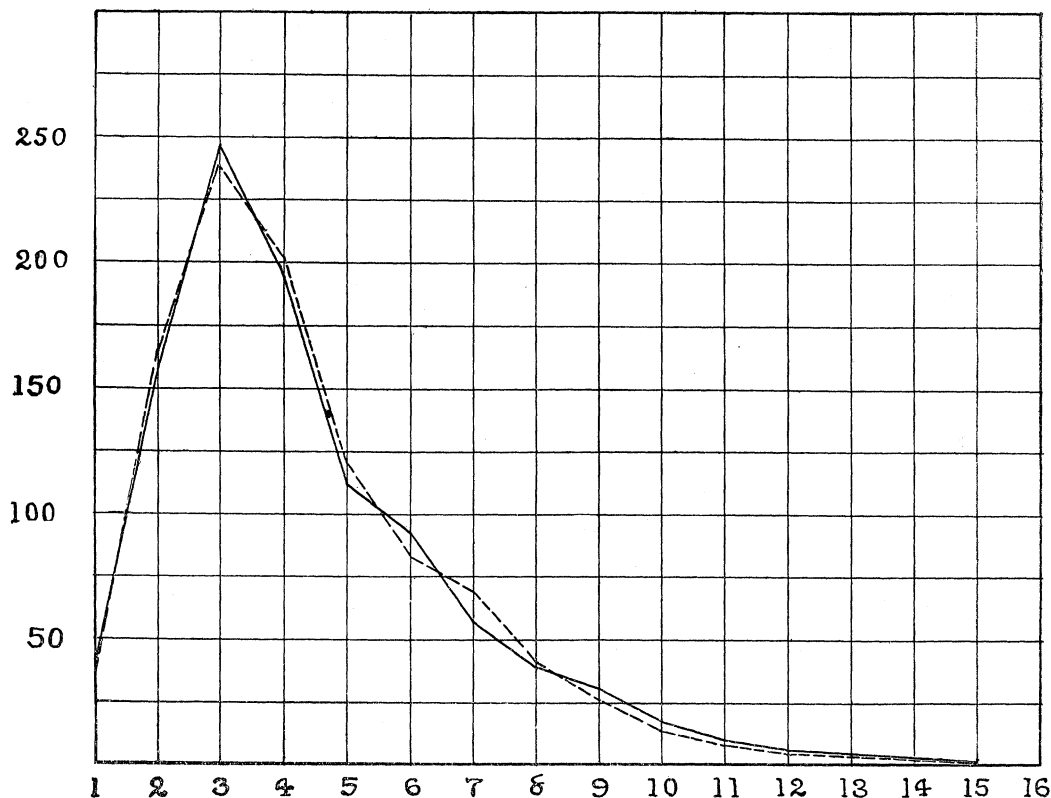


FIG. 12. — TWO GROUPS, OF TEN THOUSAND WORDS EACH, FROM DICKENS: 'OLIVER TWIST,' ———; 'CHRISTMAS CAROL,' - - - - -.

ishes for high water $2^d 22^h$ after, for low water $1^d 08^h$ before, the vanishing of the moon's declination, and the interval of the two former events appears to be independent of the solar declination. A method of graphical analysis, due to the late Assistant L. F. Pourtales of the U.S. coast survey, brings out the fact that the diurnal inequality at Fort Conger is caused by a wave that has a sidereal day for its mean period; the same thing is also shown to obtain at Port Foulke and Van Rensselaer harbor in Smith Sound, and at Thank God harbor in Polaris Bay. The general phenome-

A comparative study of the specific characters of the Fort Conger and other arctic tides with the cotidal lines, widths, and depths of the tidal avenues to the Polar Ocean, with whatever other tidal data from high latitudes was accessible, resulted in certain inferences stated in the report, and which may perhaps be tolerated here. The weakness of the tide-producing forces near the pole and a propensity to dissipate as a free wave as soon as formed, in waters of even moderate depth, are two causes operating to prevent the generation of local tides of appreciable magni-

tude in that region. The tides of the Pacific are not likely to make themselves felt in that vast expanse through a strait only some forty miles in width and less than thirty fathoms in depth, with far-stretching shoal approaches on either side. On the other hand, the relation of the Polar to the Atlantic Ocean is so intimate as to amount to identity. The continuity of the Atlantic basin has been demonstrated by soundings up to and beyond the 80th parallel. The channel between Spitzbergen and the European coast is about a

But the laws of the tides in the circumpolar seas, a *cul de sac* into which run the tides of an ocean stretching from pole to pole, and where the absence of controlling astronomical forces is favorable to tidal anarchy, can only be determined with certainty from long series of observations at stations generously distributed about the polar basin. The establishment and maintenance by Lieutenant Greeley of one such station, and his preservation of the records of observation, will be regarded as substantial services to science

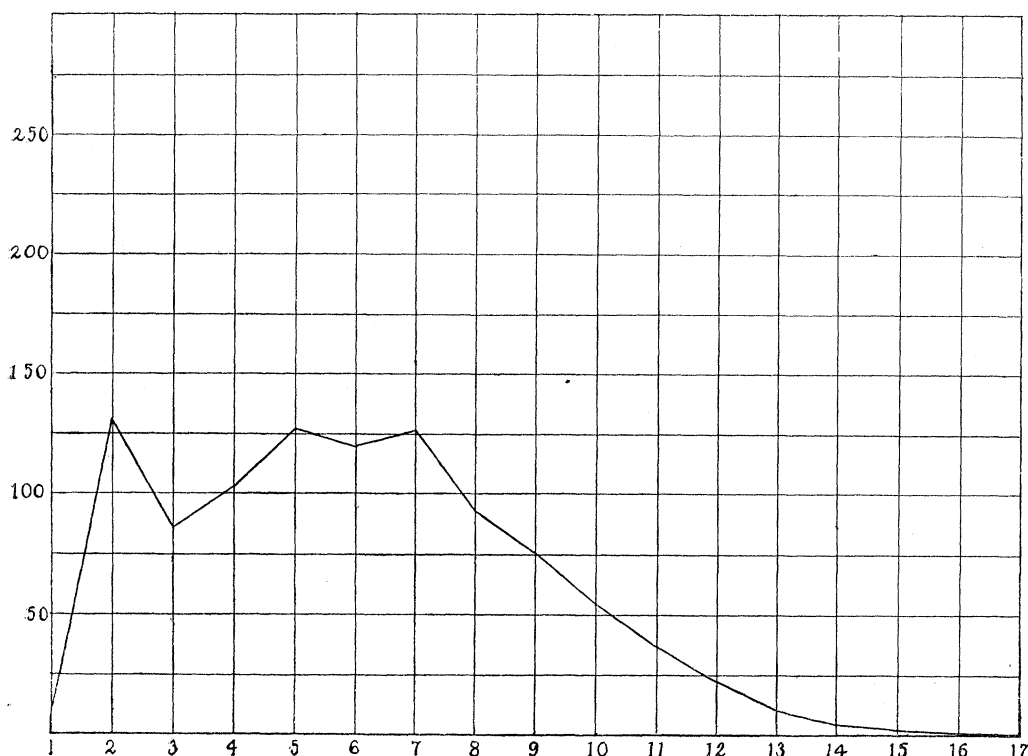


FIG. 13. — GROUP OF FIVE THOUSAND FIVE HUNDRED WORDS FROM CAESAR'S 'COMMENTARIES.'

hundred fathoms deep and four hundred miles in width; that between Spitzbergen and Greenland has about the same width, but is one, two, and three miles deep. The tides of the circumpolar seas cannot avoid forming a part of the Atlantic system. As to the tide in Lady Franklin Bay, it seems almost a certainty that it is chiefly an Atlantic tide that has flowed up through the Spitzbergen Sea, rounded Greenland, and entered Robeson Channel from the north, where it probably meets another and fainter Atlantic tide from the south, which, delayed and spent in the shallow West Greenland seas, comes into Lady Franklin Bay two or three hours later.

by all interested in this branch of physical inquiry.
A. S. C.

AGRICULTURE IN ENGLAND IN 1886.

IN outlining, in a recent number of *Science* (ix. No. 212), the reports presented by the British commission on the existing trade depression, special attention was called to the fact that it was admitted on all hands that the agricultural classes were the worst sufferers. The lower prices of agricultural produce were very far-reaching in their consequences. For this reason the latest returns as to that produce are of timely interest; and we con-

dense them from a recent parliamentary paper which shows the extent of acreage, and the estimated average produce per acre, of the principal crops of the United Kingdom for 1886. The estimate is based on returns received from about 14,000 parishes.

The figures show that during the year, England produced a wheat-crop of 58,071,171 bushels, which shows the large falling-off of 15,950,077 bushels, or more than 21 per cent on the year 1885, at an estimated average in 1886 of 26.87 bushels an acre, against 31.51 bushels in the year before. The falling-off from the average yield of an acre appears in all the counties of England except four. For Wales the estimated total produce of wheat amounted to 1,501,075 bushels, at an average rate of 21.86 bushels an acre, being .33 of a bushel above the estimated normal average. For Scotland the total produce of wheat is shown to be 1,895,652 bushels, at an average rate of 33.77 an acre, which may be compared with an average of 34.33 in 1885. The year's average, though smaller than the previous year's, is larger by nearly a bushel than the ordinary average. The aggregate results for wheat in Great Britain thus amount to 61,467,898 bushels, as compared with 77,587,666 in the preceding year, while the acreage under wheat was 7.8 per cent below that of 1885. Ireland also shows a diminution in the production of wheat, the numbers of bushels being 1,879,987 as against 2,048,103, a decrease of 8.21 per cent.

Of barley, the United Kingdom produced 78,309,607 bushels, as against 85,721,632 in 1885, and this decrease of 8.65 per cent is shared by all parts of the kingdom. The return for oats is more favorable, as the production of the whole kingdom was 169,376,088 bushels, an increase of 5.57 per cent over 160,440,907 bushels, the yield of the preceding year. In this crop Wales is the only portion of the kingdom where there is a decrease, and that is very small.

The pulse-crops are again a partial failure in many counties, and the production of beans and peas shows unsatisfactory results when compared with the normal rate of yield. The numbers for the whole kingdom, however, show an advance on those of 1885, being, for beans, 10,307,187 bushels, an increase of 15 per cent; for peas, 5,855,382, an increase of 35 per cent.

Of the root-crops, potatoes show a decrease from 6,374,242 tons to 5,835,487, a falling-off of 8.45 per cent; and of this, Ireland bears more than her share, as the returns from that country fell off 16 per cent. Wales and Scotland, on the other hand, are a little above the average. Turnips show an improvement in all the four divisions of the kingdom, having risen from 24,062,-

608 tons to 33,957,415, which means an increase of over 41 per cent. Mangold, again, shows nearly as large an increase, from 5,969,523 tons to 7,788,811 tons, which is over 30 per cent.

The hay-crop from grass grown on permanent pasture-land is shown to exceed slightly the average yield an acre in Great Britain, the total produce amounting to 5,763,235 tons, while that from clover is at the normal average of 3,311,449 tons. the total produce of both descriptions thus showing an aggregate of 9,074,684 tons. Hops show a decided gain in the year, as the yield in 1886 was 776,144 hundredweight as against 509,170 hundredweight in 1885, or an increase of over 52 per cent.

The tables show, that, on comparing the figures for 1886 in Great Britain relating to the produce of the crops dealt with, mangold, hops, and hay are the only ones showing an increase on the estimated ordinary average yield. Corn and pulse crops, potatoes, and turnips all show a decrease on the average, though in some cases they are in advance of the previous year. The returns for Ireland show a decrease, on the average, of wheat, barley, beans, and potatoes, and an increase of oats, peas, turnips, mangold, and hay.

NATURAL GAS.

In a paper on the pressure and composition of natural gas, read before the Engineers' club of Philadelphia, Dr. H. M. Chance stated that there are no records of the gas-pressure first shown by the larger wells. The recorded pressures were nearly all observed after the gas had been blowing off for some weeks, months, or even years; and the pressure then shown by a gauge is evidently no measure of the pressure under which the gas exists in the rock, for the gas soon becomes exhausted from the immediate vicinity of the well, which then draws its supply from a considerable distance, and perhaps through bands of rock of such texture — and perhaps even through the clay filling of crevices — that the pressure shown at the well may be only a fraction of the actual pressure.

Hence, while recorded pressures range from about 600 down to 200 pounds per square inch, there is every reason to believe that the actual pressures are perhaps from 500 to 1,000 pounds per square inch, or even in some cases much greater, but still being less than the maximum as limited by depth. This maximum is very much less than the pressure necessary to effect liquefaction, and the supposition that the gas exists as a liquid must therefore be abandoned.

One of the most interesting phenomena recently observed in natural gas is its variability. The analyses of Professor Sadtler, made some nine years

ago, showed that gas from wells located in districts not connected with each other was similar in composition, but that the percentages of the different gases present varied widely; and more recent analyses show that gas from wells in the same 'pool,' and even that from the same well, is subject to daily and even hourly variations in composition. When it was found that the calorific value of the fuel was subject to change from time to time, as shown by variations in temperature of the furnaces, and in the steam-pressure of boilers under which it was burnt, this was at first supposed to be due to differences in pressure; that is, in the quantity of gas delivered to the burners in the fire-box. Automatic pressure regulators were introduced, and the producing companies perfected a system by which the pressures were maintained at a nearly constant figure, yet the same variations were observed. The chemists then began to examine the gas, and soon found that it was extremely variable in composition. The following table shows the results of ten analyses of natural gas, the first four being made from gas taken from the same well at different times, and the others from the gas of different wells in different districts:—

	1	2	3	4	5	6	7	8	9	10
Carbonic acid (CO ₂).....	.80	.60	—	.40	.34	.35	.66	2.28	—	.30
Carbonic oxide (CO).....	1.00	.80	.58	.40	trace	.26	trace	—	1.00	.60
Hydrogen (H).....	20.02	26.16	29.03	35.92	6.10	4.79	13.50	22.50	9.64	14.45
Marsh gas (CH ₄).....	72.18	65.25	60.70	49.58	75.44	89.65	80.11	60.37	57.85	75.16
Ethane (C ₂ H ₆).....	3.69	5.50	7.92	12.30	18.12	4.39	5.72	6.80	5.20	4.80
Propane (C ₃ H ₈).....	—	—	—	—	trace	trace	—	—	—	—
Nitrogen (N).....	—	—	—	—	—	—	—	7.32	23.41	2.89
Oxygen (O).....	1.10	.80	.78	.80	—	—	—	.83	2.10	1.20
Illuminating hydrocarbons.....	.70	.80	.98	.60	—	.56	—	—	.80	.60
Ratio, C to H (weight).....	2.72	2.59	2.64	2.59	3.08	3.00	2.88	2.70	2.91	2.84

SUPAN'S JOURNAL OF COMMERCIAL GEOGRAPHY.

THE latest supplement of *Petermann's Mittheilungen* forms the first number of a journal of commercial geography. Prof. A. Supan, the able editor of the *Mittheilungen*, intends to give in the new periodical at regular intervals a report on the agricultural and industrial produce and of the commerce of all continents successively. The present number contains a brief introduction and the report on America. The principal feature of the new journal is the use of the results obtained by statistical observations for geographical purposes. German geographers of late apply much of their time and work to studying the mutual relation between geographical phenomena and the history of mankind. We call to mind Ratzel's

Archiv für Wirtschaftsgeographie. I. Nordamerika. Ergänzungsheft No. 84 zu Petermann's Mittheilungen. By A. SUPAN. Gotha, Justus Perthes.

'Anthropogeographie,' which gave rise to numerous discussions, and was an incentive to many researches of a similar kind. The new periodical belongs to this class of publications. Supan sets forth his plan in the introduction. He intends to give a collection of reliable data arranged from geographical points of view. Thus he hopes to give material that will be useful by its clearness, and will enable the student to investigate the history of commercial life. "Whoever intends to study the relation between man and nature," he says, "must not confine his researches to a brief period. I am convinced that the geography of civilization must be studied from an historical stand-point. Here is the place where geography and history will meet again; this is the way in which geography may become a practical science in the noblest sense of the word."

Supan arranges the statistical data contained in the report of the tenth census of the United States into four principal groups, and proves that the north-eastern states have largely an industrial population. In the central group industrial and agricultural population are almost of equal importance, while in the southern the agricultural one predominates. In the western states the influence

of the mineral resources is characteristic. Supan's discussion of the agriculture of North America is accompanied by several maps which give a clear idea of the distribution of cultivated land and of the culture of wheat cotton, and tobacco. The tables are so arranged as to show the moving of the principal district of production from east to west which began between the years 1850 and 1860. In 1850 the maximum of production was found in the southern Atlantic states; in 1860 it had moved to the Mississippi-Ohio group. At the same time the minimum moved from the prairie states to the plateaus. The agriculture of the whole east shows a permanent decrease, the northern-central and the western states a permanent increase of their relative importance, while the southern states have remained stationary. The rapid increase of the importance of agriculture which prevailed in the Mississippi and Ohio group during the last thirty years has ceased,

and in their stead the prairie states are rapidly developing.

We point out only a few of the important results Supan obtained by the geographical arrangement of statistical data and of his critical remarks on the available material. In studying the industry and agriculture of the United States, he again divides them into four groups, — the north-eastern industrial district, the southern and central agricultural district, the mining district of the western plateaus, and the Pacific district, in which agriculture prevails while mining and industry are of considerable importance. The character of the United States is still that of an agricultural country, but industry is growing rapidly upon agriculture. As compared to these, mining is insignificant, the whole mineral production being only eighteen per cent of the agricultural. As we approach the southern states, the industry decreases, while agriculture increases. Going west, industry decreases, and is a minimum in the prairie states; farther west its importance is again increasing. The north-eastern states have changed their character from that of agricultural states to industrial ones. The industry of the United States is founded upon the produce of agriculture, and every province works up its native material, — the southern states, cotton; the southern-central states, tobacco, iron, etc. The New England states form the only exception. Cotton, wool, and leather manufacture are the predominating industries, — though cotton does not grow there, — and stock-raising is of no importance. The industry of this region has the same character as that of England. It consumes for manufacture the produce of foreign countries. A map accompanying the report illustrates the distribution of industrial production in North America.

The data on the commerce of the United States do not refer to 1880, as those on production do, but are the mean of the five years 1880–84. Supan prefers this method on account of the irregular fluctuations, which are of greater importance in commerce than in production. He arranges the commerce of the seaports so as to show that those of the northern Atlantic coast are importing while the southern ones are exporting. In the interior the lake district as far as Cleveland is importing; farther west it is exporting. On the Pacific coast the northern ports are exporting, the southern ones are importing, while in San Francisco both branches are of equal value. The export of manufactures is steadily increasing in value as compared to that of agricultural produce.

The statistical data on Canada show that the proportion of the industrial and agricultural population is about the same as in the United States.

The principal difference is, that the proportion is evenly distributed in all parts of Canada, while very wide differences exist throughout the United States. Canada is now in a stage the United States passed through before the rapid development of the western states and territories. The western provinces of Canada are not yet as far developed as those of the United States, and the shifting of production to the prairies, which has been going on here for more than thirty years, has scarcely begun there.

The present volume shows that results of eminent practical value may be obtained by the application of geographical methods to sociological problems. It opens new points of view to the student of political economy, showing the close connection between man and the country he inhabits.

F. BOAS.

STARTING from the common observation that when we do hard thinking we cannot at the same time use our muscles actively, Dr. J. Loeb (*Pflüger's Archiv f. Physiologie*) has attempted to estimate quantitatively the relation between physical and psychical activity. His method was to record his maximum grip on a dynamometer; then, after a short rest, to begin some mental work; and, while engaged in this, to record the maximum grip once more. The result was, that the latter grip was decidedly less powerful, and that the difference between it and the former grip was the greater, the more difficult and absorbing the mental process. For instance: in one case the normal grip with the left hand depressed the lever of the dynamometer to 77°; while reading and *understanding* (i.e., he could repeat the substance of it in his own words) a scientific work, only to 15°; while simply reading it as so many sounds, 67°. Another gentleman (Professor Zuntz) could normally depress the lever to 69°; but, while reading a catalogue of names (requiring little mental strain), to 53°. Dr. Loeb's average maximum grip when not occupied with mental work was (mean of both hands) a depression of the lever to 85°; while multiplying one number under 10 by another such number, the depression was 81°; when the two numbers were between 10 and 20, only 35°; when between 20 and 30, only 14°. This shows very clearly how the energy given over to the mental exertion is taken off from the muscular effort. It must, of course, be understood that these results have only a general value. The method presents many mechanical difficulties; the question of attention is an important factor; and Dr. Loeb simply offers these results as a preliminary statement of his intention to work upon this problem.